

TUBERCULOSIS
IN
HISTORY

TUBERCULOSIS IN HISTORY

From the 17th Century to our own Times

By

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PREFACE

THIS book is written with one intention only; to set forth the facts, as far as they are known to me, of some of those older workers on tuberculosis whose lives contributed to the establishment of the volume of knowledge which we possess today. I have, in Parts I and II, attempted to tell the stories of the earlier British observers whose contributions were so valuable from a clinical point of view as well as from the philosophical stand-point, and to give again, in a new form perhaps, some account of the wonderful scientists in the study of tuberculosis who arose in France and on the Continent towards the end of the eighteenth and during the early years of the nineteenth centuries; I have stopped short with Villemin, whose researches brought tuberculosis workers face to face with what he proved to be an infectious disease, like glanders or typhoid fever, and gave such a splendid finish to the efforts of Laënnec and Bayle.

And yet, while avoiding the "Moderns," many of whom I had known personally and of whom I felt it to be too soon to write, I was still aware that my work could not be regarded as complete without at least an attempt to bring it in some degree up to date. The result was Part III, in which I tell the tale of Edward Trudeau's gallant and successful fight against the disease and of the glorious achievements of Robert Koch in discovering and isolating the germ and making it possible for us to contend with a definite though redoubtable enemy.

Finally I must here express my grateful thanks to the many that have helped me; to the Royal Society of Medicine, before the Historical Section of which many of the stories have been told and to the authorities of which I express my grateful thanks for their kind permission to republish them; and to the Director of the Wellcome Historical

Medical Museum, Dr. E. Ashworth Underwood, for very kindly allowing me to use the photographs illustrating my work, which are from the Wellcome collection. I would specially mention that two of the photographs, those of Sydenham and of Morton, are from contemporary portraits now in the possession of the Wellcome Historical Medical Museum and therefore convey to the observer the very traits that were to be observed at the time.

S. LYLE CUMMINS

*Thaxted,
Essex*

December, 1948

INTRODUCTION

It is a great privilege to be asked to write the introduction to this book by my friend, Professor S. Lyle Cummins. Certain of the chapters formed the subject of several papers which the author read before the Section of the History of Medicine of the Royal Society of Medicine when I was president of that section. After hearing them, the wish was generally expressed that they should be published in book form; all students of medical history will be gratified that Professor Cummins has now acceded to this request and has written some additional chapters.

Professor Cummins is an authority on tuberculosis, and his work as Professor of Tuberculosis in the Welsh National School of Medicine has advanced knowledge. Now that he has entered on the historical study of tuberculosis, his experience and learning render him competent not alone to describe but also to appraise the work of the early pioneers in the field. In this respect his book may be compared to the *History of Physiology* by Sir Michael Foster, written by a physiologist; to Sir George Still's *History of Paediatrics*, written by a paediatrician; and to Dr. James Collier's *History of Neurology* (FitzPatrick Lectures), written by a neurologist. When experts study the history of a branch of medicine which they have made pre-eminently their own, their writings are for this very reason the more authoritative and valuable. Such expert knowledge illuminates the pages of this book.

Part I is devoted to the British School of phthisiologists. Here Professor Cummins has written an account of the life and work of nine British physicians, pioneers of tuberculosis in their day and generation. The period of time covered is from the seventeenth to the end of the nineteenth century, a mere three hundred years, but in that brief space great discoveries were made of which British medicine has every reason to be proud. This, as Professor Cummins notes, was the age when

scientific observation and experiment were first applied to the study of disease, and in which the old theories of the ancient medical philosophers were displaced. This change was initiated by William Harvey, bred in the light of the Renaissance at Padua, and was reflected in the study of pulmonary tuberculosis, as well as in other branches of medicine.

First of all comes Christopher Bennet, who died a victim of the disease which he had so ably studied. Then we meet Thomas Willis, whose anatomical knowledge fortified him for pathological investigation, and the "incomparable Sydenham," whose writings disclose the influence of environmental conditions on "consumptions." Richard Morton ranks pre-eminent as the author of the first systematic treatise on consumption, *Phthisiologia*, published in 1689, a great work, remarkable in its description and findings for the time at which it was written. As an example of this Morton's own words may be quoted :

"Yea, when I consider with myself how often in one year there is cause enough ministered for producing these swellings, even to those that are wont to observe the strictest rules of living, I cannot sufficiently admire that any one, at least after he comes to the Flower of his Youth, can dye without a touch of a Consumption."

This antedates by two hundred years Virchow's dictum that everyone shows, at last, some trace of tuberculosis.

In speaking of Benjamin Marten, Professor Cummins follows Dr. Charles Singer in acclaiming him as the enunciator of the germ theory of tuberculosis, and emphasizes Marten's appreciation of "contact" infection, truly, as he observes, "almost an inspired statement." Professor Cummins, as a pathologist, in his study of William Stark well brings out this physician's account of the gross morbid lesions present in pulmonary tuberculosis. He notes that Matthew Baillie confirmed Stark's findings. Baillie was an accurate pathologist, and like Bennet succumbed to pulmonary tuberculosis. This list of exemplars concludes with James Carson, the Liverpool physician, who first performed an artificial pneumothorax; George Bodington, the first pioneer in sanatorium treatment; and William Budd,

the Bristol physician and epidemiologist, famous for his work on cholera and typhoid fever, who in 1867 published in the *Lancet* his memorandum that pulmonary tuberculosis was a communicable disease. It was received with surprise and even scorn at the time.

In the second part of the book Professor Cummins describes the work of the early continental phthisiologists. For this purpose he chooses seven great exemplars. Auenbrugger, who introduced percussion into medical practice; Corvisart, Napoleon's physician, who made known the work of Auenbrugger after it had been forgotten for thirty years, and taught medicine to Laënnec and encouraged his early work; Bayle, Laënnec, Piorry and Villemin. Bayle, who accurately described early cases of pulmonary tuberculosis, was "a very exact observer, but not an inspired thinker." He also fell a victim to tuberculosis. Bayle, we learn, was at first a poet. There has been a number of poets who abandoned medicine for poetry, others who worshipped at both shrines of Apollo, but few like Bayle who renounced poetry for medicine and destroyed his poems as a burnt offering to Aesculapius. After Bayle, Professor Cummins tells anew the great and pathetic story of Laënnec's life; he discovered the stethoscope, and died another victim of tuberculosis. A short account follows of Piorry, the discoverer of mediate percussion, and then comes one of the most interesting chapters of the book, which deals with the experimental work of Jean-Antoine Villemin. Villemin's work has been overshadowed by Koch's discovery of the tubercle bacillus, and here Professor Cummins restores him to his rightful place as a pioneer in the experimental pathology of tuberculosis. Villemin's discovery led almost immediately to the excellent work of Burdon-Sanderson on the inoculability and development of tubercle, initiated by Sir John Simon and published in his Medical Reports to H.M. Privy Council in 1867 and 1868. Professor Cummins concludes with interesting accounts of the life and work of Trudeau and of Robert Koch. Throughout the book the author skilfully traces the successive stages, which have led to more complete knowledge of a prevalent disease.

In this summarized account I have picked out the plums from Professor Cummins' cake, but only in order to show what an excellent cake it is. The medical historian, the specialist in tuberculosis, and the general reader can equally enjoy this book and after perusal will feel grateful to Professor Cummins for writing it.

ARTHUR SALUSBURY MACNALTY

PART I

The British School

THE BRITISH SCHOOL

OF the coming of King James I from Scotland to the throne of England in 1603, Trevelyan writes: "The land seemed bursting into bud to welcome him, growing greener each day as the ever-increasing train of courtiers wound slowly down out of the north country into the midland valleys; through shouting market places where the masque of welcome and the corporation with its address were lost in the press of men; by ancient steeples rocking with the clash of bells; along roads hedged with countrymen who had come on pilgrimage across whole counties. There was hunting of the stag through the neighbouring parks where His Majesty might discourse learnedly to the foresters on the Art of Venery and show how your Scotchman will blow a mort; while at night in private mansions the royal entertainment witnessed to the solid magnificence and loyalty of England." Such was the welcome he got from his newly inherited land. In another paragraph Trevelyan describes his appearance. "The man on whom the English first set eyes was by no means contemptible in person in spite of grossly coarse manners. In the prime of life, of middle height, a good horseman, devoted to the chase, drinking hugely but never overcome by his liquor; he employed a pretty wit and a wealth of homely images and learned conceits in free and familiar discourse with all." Such was the welcome given to the new King and such was the man himself who was now to undertake the supreme control of this land of hidden fires and undisclosed wonders, destined to work on for the future, while His Majesty killed his stags and took up his favourites for the beautification of the moment of time in which he lived and reigned.

For this was the age when the old theories of Hippocrates and Galen were being tried out in a new way and found wanting; the age of experiment which was to send medicine forward in spite of the clinging of the vast mass of the profession to the

old ways and the old formulae. William Harvey (1578-1657) was already at work and destined to discover the circulation of the blood in 1616 and to publish it to the world—or to that limited and precious portion of the world that was ready to hear him—in 1628. Christopher Bennet was then eleven years old, with all his period of study at Oxford before him. Surely in his medical years at the University he not only heard of Harvey's great discovery but perhaps met the Hon. Robert Boyle and his assistant Hooke, both thoroughly in touch with the most recent work. It may be that he encountered Thomas Willis, four years his junior, and perhaps Sydenham, though the latter was seven years younger and so probably entered the University after Bennet had left. But it is evident that there was abundant opportunity to be influenced by the great example and the distinguished learning of Harvey himself which must have permeated the whole of medical study at Oxford as well as at Cambridge. The publication of a book such as the *De Motu Cordis et Sanguinis* must have reached the teachers and, through them, the pupils and students of medicine at both Universities. There could be no better study for one destined to write about consumption of the lungs than that offered of the circulation of the blood.

CHRISTOPHER BENNET (1617-1655)

CHRISTOPHER BENNET was born in 1617, the son of John Bennet, of Raynton, in Somersetshire. All traces of the father have disappeared with the flight of time, but he was at least a man of sufficient property to send his son to Oxford, for we learn from the *Dictionary of National Biography* (Leslie Stephen, London, Smith Elder & Co., 1885) that Christopher was entered at Lincoln College in the Michaelmas term of 1632, became a B.A. on May 24th, 1636, and a M.A. on June 24th 1639. After this, no doubt, he turned to the study of medicine, but we can find no record of this in his Oxford career. It is worth noting that he became incorporated at Cambridge shortly after this date, becoming a member of Catherine Hall, and took his M.D. there in 1644. Was this a reaction to the fact that under Laud's guidance the whole religious character of Oxford had been gradually changing from a puritan to a High Church complexion? Or was it that, with the succession to the throne of Charles I, an event which took place in 1625, and the convulsion of the Civil War, Bennet decided to transfer his medical studies to Cambridge? The two influences seem to have been inextricably mixed. Again we quote Trevelyan: "The troubles began before another year was out. Seven times the trees were to bud on the banks of the Cherwell, and again these Lords and Ladies would inhabit these same colleges of Oxford, acting plays, composing sonnets, aiming epigrams, fighting duels and making love. But in 1643 the Puritans were no longer the fools upon the stage but myriads of armed and angry men weaving far and wide over England the net of destruction for Oxford and its inhabitants." It is significant that it was in 1636, the very year when Bennet took his B.A. degree, that in the words of Trevelyan, "the troubles began." Three or four years later they must have been at their worst! It may perhaps be fair to assume that he changed over to Cambridge because, to him and his like, Oxford had become impossible,

but, of course, he may have spent some of the eight years in studying medicine abroad. Bennet was admitted as a candidate of the Royal College of Physicians in London on July 11th, 1647, and became a Fellow on December 7th, 1649. He was Censor in 1654. It appears that he first practised in Bristol, a city noted for its high phthisis death rate, and afterwards in London. Here he was to acquire "a considerable reputation" according to the *Dictionary of National Biography*, and probably worked among a group of people a large number of whom were phthisics. He died of consumption on April 39th, 1655; a victim to the disease which he had so closely studied both in his numerous patients and in his own case; especially in this last for there is no disease in which a physician turns from the theoretical to the practical so surely as in the disease from which he himself suffers.

And yet, reading his book, *Theatri Tabidorum Vestibulum*, of which it is said that "its most valuable feature is the constant reference to cases observed and to dissections; not to authority," we still find a great deal of material which may have seemed practical in his day but which is very theoretical to the modern reader. Some parts, however, are eminently practical and we shall endeavour to lay particular stress on the portions best worth quoting. The book, *Theatri Tabidorum Vestibulum* or *The Nature and Cure of Consumption*, was translated into English in 1720. From the preface we quote the following passage :

"The author of this work was greatly eminent in his time for Practice and particularly in the Distemper here treated of, for which he seemed to be fitted in an uncommon manner; being himself naturally consumptive and with Difficulty supported under it for many years together; for that he had not only great Opportunities of becoming thoroughly acquainted with the Disease from a large Practice but from a long experience of what passed within himself. Thus instructed, all his Acquaintance and the most eminent of the Faculty in particular, were desirous for his writing upon the subject; and the following Work seems to have been extorted from him by their Importunities."

Nevertheless the book has to be very carefully studied to become alive to its bearing upon consumption as we know it today. It is written with great sincerity, but with perhaps too much certainty that the views put forward are correct!

No doubt this certainty was a distinct asset to one prescribing remedies for sufferers from the malady and to a man who had to apply his remedies to his own disease as well. Nothing helps the professional man more than an unswerving confidence in his diagnosis and in his treatment, and this Bennet appears to have had. We now have a clear conception as to what tuberculosis is; we have that definite advantage, in most cases, of the finding of the tubercle bacillus to make clear the diagnosis; we have the X-ray picture to guide us and to show all that a shadow of the lesion can show as to the date of onset and the increase or recession of it in point of time; and we have the tuberculin test; that test so valuable for the groups and so unreliable in its significance for individuals! We can diagnose the case much earlier than Bennet could and, by so much, we can speak with greater assurance than he could on the subject of prognosis—but can we prescribe any treatment that guarantees a cure? Not yet! We can still call on the trust and confidence of the subject and still we have to rely—not always with success—on a satisfactory issue that may, or possibly may not, be forthcoming. So let us not be too harsh with Christopher Bennet or too much inclined to laugh at him for what was current opinion in his day, but rather let us quote him and try to understand him in his writing upon what still holds us guessing—tuberculosis.

First of all we shall quote the more definitely Hippocratic and therefore the most doubted of his aphorisms. We shall revert to his more practical recommendations later. “From the Incitement of the Blood which, for the most part, is the Efficient of Secretion,” he says, “and from the Expression of Serum from the Lungs, moderately bedewing them, a frequent Spitting will sometimes be raised; but if a Person is overflowed with a great Profusion of such Serosities, which induce a general Laxity, a more protracted Ptyalism will succeed. From the collection of more dense Fluids, the Bronchia will be

stuffed; from a thinner Matter insinuating inwards they will be distended or their Lobes, being in some wise infected, will rise upwards; and lastly, from a settled and most gross (not to say Malignant) instufflation, will arise a very dangerous Asthma. . . . But the Exhalations from the solid Parts flow along with the Blood; and those Passages, even through the Heart, sometimes happen without any material Injury to the Constitution: Yet, in my opinion, the Universal Taint of such Exhalations will sooner bring on that Debility, in the course of an obstinate *Phthisis*, than if the Blood had failed in its Consistance."

Here the word Asthma is used for a severe coughing and not merely to describe the spasmodic variety as we know it today, but the occasional complete recovery is well set forth. "More than once likewise have I taken notice of the return to strength to phthysical Persons after they have cast up a great deal of Filth by Expectoration; and of which a Recollection would throw them into their former Langours." Some of these bettering patients may not have been suffering from genuine phthisis at all and some may have made a temporary halt in their consumptive process. He speaks of all as "in the course of an obstinate *Phthisis*," but it is evident that he is aware of differences between one case and another as to their prognosis. Perhaps there is nothing truer than that persons who have got, for the time being, into a fitter state may be thrown back "into their former Langours" by the mere *recollection* of what they suffered from previously. They need, moreover, to be occupied with their improvement, not brooding over the memory of their worst days.

Bennet lays a great deal of stress on the sputum; rightly so for our uses today, but especially for a time when the methods of examination were so few. To quote him on this subject we extract the following passage: "Those Physicians seem to be wanting to themselves, although in other respects the most sagacious, who slightly examine the Nature of what is expectorated. For a great many observing purulent matter ejected by coughing after Blood-spitting pronounce that Ulcers or Abscesses are aforming and which necessarily discover an approaching *Phthisis*. And this error they have fallen into by



*Hospitij quicumq; petis, quis Incola tanti
Spiritus, Egregia hinc, Consule scripta dabur
Chir: Terne, M.D. C.L:
E ombor*

CHRISTOPHER BENNET (1617-1655)

searching into the Doctrine of Hippocrates only by halves whereby they are tenacious of their own notions and neglect due Assistance to the great Prejudice of the Sick."

With all due deference to the author, however, the physician who gave anything but a poor prognosis in such a case would be taking a grave responsibility; "purulent matter ejected after blood-spitting" seems to us often a sufficient indication of gross lesions in the lungs. We, too, have searched Hippocrates, but perhaps only by halves and certainly without much success. We find that he says (on page 739 of the *Aphorisms* in Adams's *Genuine Works of Hippocrates*, London, 1869) that: "In persons who cough up frothy blood, the discharge of it comes from the lungs," that "from a spitting of blood there is a spitting of pus" and that "from a spitting of blood arise phthisis and a flux"; these last on page 764; but we have failed, like the careless physicians spoken of, to discover anything except the sentiments quoted which seem, if anything, to favour the latter! We thoroughly agree with the writer, however, in his condemnation of doctors who "slightly examine the Nature of what is expectorated" and we commend his thorough description of the various kinds of sputum which we quote in some detail:

"White and frothy A frothy spittle is mixed with air. If this spittle be brought to trial it will be found to be very light, consisting chiefly of a watery Pellicule inflated with air."

"Of what is of a yellow colour. The constituent Matter of yellow Expectoration is very probably from a bilious tincture but hath been so weakened in the Blood before secretion that it becomes deprived of its taste at least."

"Blakish, Bluish and Rust-coloured. Among these the most remarkable that do occur are the bluish, the rust-coloured and the blakish Spittles, which do not, as many imagine, take these variations from anything which is drawn in with the Breath . . . but from a taint of the viscera and vessels."

"Of a salt Spittle. A sweetish Spittle. When I practised in Bristol, a city that is built much over Vaults and Cellars,

and where Consumptions are very frequent, I have met with some who with much Ease brought up a sweet Spittle for three years together and have been totally enervated and wasted."

"Of a Dirty, Ash-coloured and Purulent Spittle. Amongst all the Excretions the most vapid and feculent Spittle is of an Ash-colour and dirty, like soft clay; but the less adhesive Spittle of Consumptive Persons is a symptom of a declining natural Heat and of approaching death."

Of course all this was written in the belief that sputum came directly from the blood into the lungs. As Bennet says: "That the Pulmonary Artery with its Divarications receives the Blood-Recrements and thence detaches them into the Branches of the Wind-pipe is a matter quite out of Dispute." But, even with this erroneous belief the *observations* were very good and this close study of the sputum is worthy of a thoroughly good physician. We must remember that he was dealing with a macroscopic examination of the "spittal"; not with the microscopic that has become a rule today.

Bennet, as was natural at that time, firmly believed in the hereditary nature of tuberculosis. He noticed the preponderance of the disease among the children of tuberculotics and in the absence of any means such as the tuberculin test and the presence of tubercle bacilli in the sputum of the parents or associates, drew the only conclusion that was then possible. "Nor are there wanting such as bring their consumption with them into the World, whose Parents have died Valetudinarians or Consumptive, abounding with a cold or an immature Semen or perhaps over-run with an acrimonious Heat. . . . These Persons have sharp shoulders which are therefore called *πτερεγοιδες* (wing-like), a contracted Thorax, a narrow and low Chest, a thin, long neck, a flaccid Tone of all the Parts about the Breast and a very flabby Contexture of the muscles all over the Body." This is the phthisical type referred to by Hippocrates (*The Epidemics*) and requoted by Galen and everybody else down to our own time; but he makes the description very much more forcible by referring such persons to *those inheriting tuberculosis*, for it is quite clear that the conveyers of such signs were already

suffering from the disease, not merely likely to get it! Bennet, while believing that tuberculosis was often hereditary, still appears to have held with the frequency of a non-inherited type; he may have observed that the former were often the most resistant. "They who have received an indelible Impression of a Phthisis from their Parents; although they cannot be restored, yet they will wear a while." He describes coughs of a fairly mild kind, but goes on:

"More vehement and plentyful Affluxes are known: By more copious casting up a frothy Blood of a clay colour, with Hawking and Coughing less painful if from the lungs but of a blakish or deep red colour with a more painful Coughing if from the cavity of the Thorax: if from the great Arteries, regurgitating into the Mouth by Gushes and Intervals; but if it proceeds from the Fibres it distills gradually but with a sense of Titillation."

Here we feel for a poor fellow trying to express, with confused theory but with deadly sincerity, from observed facts, his own experiences of massive haemorrhage or slight and transient staining of the sputum.

He proceeds to enlarge on the signs and symptoms:

"The Signs of a Phthisical Consumption from what source so ever . . . as to the present Cure are:

"A very aggravated cough the violence of which stains Spittle with P^r medⁱ then a filthy pus of an ash colour . . . and lastly medicine vents or Fragments of the Lungs themselves; a st^r Hall, C^o and the utmost Difficulty in Respiration; a p^righting in t^h reast and a prickling Soreness of the Paps, esp^o time of coughing; a defect of all the functions; Tol^o relaxed er from the Blood universally tainted by a Distill^o a Flux of the Bowels . . . a general Dryness of t^h ex^o . . . a stretching Pain in the right or left side upon t^h down by reason of some Adhesions of the lungs upon one side or other of the Pleura; . . . That Countenance called Hyppocratic or the very image of Death . . ." He adds, with what implications we can imagine, "That Langour which slowly steals upon Consumptive

People without any decay of the Lungs . . . unless Remedies immediately take place (which they seldom do!) is mortal."

He asserts that "the Blood is corrupted from inordinate Motion and Incalescence as the Aliment is from an erroneous Fermentation; but both these may be prevented by a due administration of the Non-Naturals." (These, in the language of the medical philosophy of the ancients, were defined as things which entered as necessities into the *existence* of man but not into his *composition* or into his nature). They are as follows, to put it as briefly as possible: Food, sleep, exercise, avoidance of strong purges, the laying aside of care, and the breathing of "an open fresh and kindly air." Surely these six things are as natural as can be! But whether we call them natural or non-natural they have great importance and Bennet, though he looks on them as too well-known to require recapitulation, gives the following practical measures for their use:

"It is also impossible to give general Rules with relation to meats and drinks because the strength of the whole Body and particularly of the Stomach . . . are to be weighed with great exactness. A food that is of easy digestion and nourishment is most suitable for weak Persons."

"It is proper to limit Sleep to a due measure in all Distempers but more in those which regard Respiration.

"The daily Motions of the Body are chiefly to be considered and, at the first accession of Symptomption exercise ought to be moderate . . . But the most braletudināse that opens the muscles of the Breast may be sonimmature -ised to those whose organs of Respiration are of a . . . These ne and cold Temperature, as drawing the Bow, exercise of the Pikes, etc."

"The manner of lying at night is by no means to be neglected. Lying on the left side is most hurtful to those who have the Lungs burdened by a thin Suffusion and they who lie on their Backs are most incommoded, if they are offended by a Distillation."

"Venery must be very sparingly allowed."

"In order to keep the Pores moderately open the Patient

ought to wear thicker Cloaths or more Garments . . . To such as are thin and shrunk, instead of Linen which they wear next the Skin, let such put on the same things made of fine Woolen such as is called Flannel; nor are they to be so often shifted . . . amongst the Wollen coverings they seem to me much the best which are made from the Wool which grows on the Flanks of the Animal . . . But a warm, thin and dry air, a sandy or chalky soil and hilly Country is best for a flaccid and tender constitution."

So speaks Christopher Bennet and we can agree with him in his application of what he is pleased to call the Non-naturals. He also gives a number of tips and prescriptions by which the patient "may be either restored to present Health or a longer Enjoyment of Life" but we do not think these worth recapitulating as the treatment of phthisis by the ordinary drugs is no longer much favoured and the special drugs that may perhaps be worth experimenting with were still, of course, unknown in Bennet's time. As we have said before there is a great deal of Hippocratic theory which is best left to be studied by those interested in such things—but we believe that we have benefited by a study of Bennet's practical methods and that others may, perhaps, do the same.

I have ventured to speak of Bennet as, perhaps, compelled to transfer his medical studies to Cambridge—or some other school of medicine abroad—and to take his medical degree at Catherine Hall, Cambridge. It would seem, however, that to one delighting in the presence of the Court and prepared to continue at the University, there was nothing to prevent the study of the medical profession at Oxford itself. Thomas Willis seems to have been such a one; devoted to Oxford as his father had been before him; both "retainers," the one of St. John's College and the other, as he grew up to man's estate, to the Canon of Christ Church. Both remained at or near Oxford through the time of its occupation by King Charles and his Court and both must have been in the Town and University in 1646, the year of the siege, as the older Thomas was killed

there in that year and the younger graduated as a Bachelor of Medicine. And it must have been possible to get a thorough education there for Willis was destined to become one of the really brilliant physicians of his time and greatly to enrich medicine and physics by his contributions. On the other hand, Sydenham, a member of a strictly "parliamentary" family and, probably, much opposed to the turn events were taking at the Oxford of his time, was constrained to move out of it and, with his brothers, to take up the challenge and oppose the King's party in arms. It was not until Charles and his following had been beaten out of Oxford that he returned; also in that year of crisis, 1646, to take up his studies as well as to hold office there.

THOMAS WILLIS (1621-1675)

THOMAS WILLIS was the son of Thomas Willis and Rachel Howell, his wife, and was born at Great Bedwin in Wiltshire on the 21st of January, 1621. His father was a farmer at Church or Long Headborough, Oxon, a retainer of St. John's College. He was later steward to Sir Walter Smith of Bedwyn, retired to North Hinksey, near Oxford, of which place his wife was a native; and was killed during the siege of Oxford in 1646. The young Thomas was educated at the private school of Edward Sylvester in Oxford. He became retainer to the family of Dr. Thomas Iles, Canon of Christ Church. His university career was as follows: on March 3rd, 1636/37, he matriculated from Christ Church, being then sixteen years old; he graduated as B.A. on June 11th, 1639, and as M.A. on June 18th, 1642. He served in the University Legion, a corps devoted to the King; and it is presumed that he studied medicine at the same time. He graduated M.B. on December 8th, 1646, and started practice from a house opposite Merton College. Dr. C. G. Douglas, C.M.G., F.R.S., to whom I am much indebted for

this and other information, writes as to the medical teaching at Oxford in the seventeenth century: "At the dates you are concerned with I do not think that there was any medical school in the strict sense at Oxford. Medicine was probably studied elsewhere (even at Leyden or Padua) and the Oxford D.Med. subsequently given by incorporation." It appears to the author, however, that Willis, that devoted child of Oxford University, must have made some sort of arrangement to get his early medical training locally. The *Dictionary of National Biography* says of him that "he served his King in the University Legion and studied medicine," as if he had carried out both duties at the same time. The interval between the taking of his M.A. and the conferring of his M.B. was only four and a half years, hardly enough to have allowed him to travel much, and the university must have been in a very disturbed state at the time and the University Legion pressing in its claims. While Dr. Douglas is undoubtedly right about the usual teaching for medical subjects in the years that were passing we may, perhaps, regard Willis as an exception to the rule for the reasons given, but he may, of course, have slipped away to some foreign school for a short time. Wherever his medical training was carried out it must have been thorough to produce such brilliant results. It is interesting to learn from the D.N.B. that he was a colleague of Dr. Lower (1631-1691), ten years his junior, the man who noticed that the blood entering the lungs was dark and the blood leaving them bright red and who concluded from his experiments that the entrance of fresh air into the lungs was essential to the maintenance of life. Douglas Guthrie writes in his *History of Medicine* (1945) of "a number of Scientists in Oxford, mostly physiologists," holding "meetings for the advancement of their interests." They included Thomas Willis, Francis Glisson, Richard Lower, Robert Hooke, John Mayow and Christopher Wren; all great men who fulfilled the object of the Royal Society, viz., "the improvement of natural knowledge." This founding of the Royal Society may be verified by a reference to Shuster's *History of Science at Oxford*.

In June, 1660, Willis was appointed Sedlian Professor of Natural Philosophy and, on October 30th of that year he was

created M.D. He was undoubtedly a very distinguished physician. He published, amongst other things, his great work, *Cerebri Anatomae Nervorumque Descriptio et Usus*, and described the anastomosis, at the base of the brain, of the branches of the vertebral and internal carotid arteries, the "Circle of Willis." He discovered, too, that certain cases of wasting and frequent urination were associated with a sweet urine, thus establishing the first great forward movement in the aetiology of diabetes. His contribution to the study of phthisis is contained in a special chapter of his *Practise of Physick*, printed in London for T. Dring, C. Harper and J. Leigh, and "to be sold at the corner of Chancery Lane and the Flower-de-Luce, over against St. Dunstan's Church in Fleet Street," of date 1684 (Part II, Sec. I, Chap. VI, p. 28), a publication that took place nine years after his death in 1675.¹ This chapter is entitled "Of a Phthisis properly so called or of a Consumption arising from a Fault of the Lungs." Great stress is laid therein on the importance of pulmonary disease. "For in the Lungs rather than in the Heart or Brain the threads of Life are spun and there they are oftenest broken." He goes on: "A Phthisis . . . is usually defined to be a pining away of the whole body takings its rise from an ulcer of the Lungs but I have opened the dead bodies of many that have died of the Disease in whom the lungs were free from any Ulcer yet they were set about with little swellings or stones or sandy matter throughout the whole . . . wherefore Pthisis is better defined 'That it is a withering away of the whole body from an ill formation of the Lungs.'"

This, though at first sight rather tautological, is still very important, expressing a real difference in pathological findings; the cases breaking down into cavities on the one hand in contrast with those infected with miliary tubercles or those of a more chronic nature but without cavitation. Those lungs that were "set about with little swellings," lungs infected with miliary tuberculosis, in fact, were probably the great majority of the non-cavitated ones; the most rapidly fatal type of all. This

¹ In the translated edition this chapter appears to differ slightly from that above mentioned, and though of the same date and by the same publishers, is stated to be Part II, Sec. I, Chapter V, p. 25. The text is not exactly the same but it is to the same effect. The quotations are from both.



Thomas Willis M.D.

THOMAS WILLIS (1621-1675)

paragraph also lays great stress on the "withering away of the whole body," a very proper emphasis on the original meaning of the word *phthisis* and different from what we now imply when we speak of tuberculosis of the lungs; quite different and yet, in fundamentals, not so very different after all for although the aetiology is now distinct and wasting due to the tubercle bacillus is separated from all other types of wasting, yet it is the case that the term "wasting," the essential *phthisis*, is nearly always tuberculous in origin. Willis, in fact, mentions the wide applicability of the word *phthisis*. "Of this disease, as *there are many kinds and differences*, so two chiefly and more obviously occur; *Tabes or Phthisis Dorsalis* commonly so-called and *Consumption of the Lungs*." It is to be noted that he picked out the two, *tabes dorsalis* and pulmonary consumption, both due to the tubercle bacillus, as those that "chiefly and more obviously occur." Even then, although the tubercle bacillus was not yet known or thought of, the word "*phthisis*" was being dragged from its original meaning to be used for what was actually tuberculosis! He was, of course, excusably wrong as to the aetiology of both, attributing, for instance, spinal caries to "immoderate venery" and "from a distillation into the Spine of the Back." As to the lung lesions he speaks as follows: "The Ancients, following Hippocrates, . . . assigned (1) a Catarrh and (2) the breaking of a vein; to which some have added an Empyema . . . For what is vulgarly inferred, that Phlegm falling from the Head into the Lungs and abiding there putrifies is most commonly the cause of a *Phthisis* . . . we have formerly intimated to be altogether erroneous . . . Anything that is an Enemy to the Lungs creeps in and is admitted chiefly by the Trachea or by the Pneumatic arteries; yea and sometimes by the nerves . . . but nothing by the veins or Lymph ducts whose whole function is to carry back or away the Blood or Lympha and to leave nothing at all . . . A pernicious pollution by the Entrance is clear from hence because the moist air . . . repleat with fumes or abounding in malignant vapours doth frequently induce the consumptive inflammation." This gives, perhaps, an erroneous or an incomplete expression of mode of entry of germs—we must remember that they were then unknown

—but what a remarkable refutation it is of the once common belief that catarrh and phthisical coughs were due to the descent of matters from the head and their putrifaction in the lungs; what a correct conception of the entry of infected air and its contents through the trachea; what a remarkable utterance of the fact that substances in suspension in the air (he calls them fumes) must first enter the lungs through the trachea or the pulmonary arteries and that the veins and lymph ducts merely carry back the blood and lymph! He did not, of course appreciate the fact that the blood and lymph returned by the veins and lymph ducts might contain the germs of tuberculosis and that these might be filtered out by the lymph glands but at least he got the *physiology* of the parts correct.

To return to the lung lesion, Willis writes of it as follows: "But sometimes it happens that there is one Ulcer or Hole, or haply two, formed in the lungs and the sides grow callous round about so that the matter being there gathered together is not conveyed into the mass of blood but is daily expectorated though in vast plenty. They that are so affected, as if they had but an issue in the Lungs, although they cast up much spittle and thick yellow matter every morning and sometimes all day, yet they live well enough in health, they breathe, eat and sleep well, are well in flesh or at least remain in an indifferent habit of body and frequently arrive at old age; in so much that some are said to have been consumptive thirty or forty years and have prolonged the disease even unto the term of their life (for that cause not being shortened). And in the mean time others, who cough and spit less, within a few months fall into a hectic fever and in a short while are hurried into their grave." This emphasis on the relatively long survival of old cavity cases and the rapid death of some who, "though they cough and spit less," still appear to be doomed from the start, is very much what we might describe today if we were asked to write a note on the time of survival of cases of consumption. If a man has resisted phthisis long enough to develop chronic cavities that have grown "callous round about," as Willis puts it, he may often go on for a considerable time in fair health or even survive long enough to die of some other condi-

tion! Others, in the meantime, go rapidly to their end without power to check the onset of the infection. It seems probable that individual differences are frequently due to the *age* at which infection first took place and to the *degree* of this early infection but, of course, there is the competence of the infected body to take into account and the amount of allergy as well as of immunity developed. The allergic man, if not of the hyper-allergic type associated with rapid disease, is often the relatively immune man capable of developing chronic cavities. And the *race* of the man counts also, not because any race is peculiar in its response to tuberculosis but because some races have been much longer and more intensely in contact with tuberculosis while others are only beginning to acquire the relative immunity characteristic of peoples of well-tried stock.

Willis speaks of the sending of phthysical patients to the country or to France with a great deal of caution. "Wherefore all our villages near London which enjoy a clear and open air are esteemed as so many Spittels for consumptive Persons notwithstanding all do not alike receive help from such a change of place; for many, after passing to France or to country Villages do in these places rather find their graves than Health. And therefore London is not presently to be foresaken by all Phthysical Persons; for I have known many obnoxious to a cough or consumption to have enjoyed their health much better in this smoaky air than in the country. So that for the cure of the same Disease while some avoid this City as Hell some fly to it as an Asylum."

He sums up very acutely the different stages at which consumption is likely to be encountered. "Nevertheless it is fit we observe the divers states or distinctive signs which certainly belong to it: (1) When it is merely a cough; (2) when it begins to degenerate into a Phthisis; (3) when it is a perfect and almost desperate Phthisis. For which things duly designed like the Prognostic of the above-mentioned affections will be very apparent."

"(1) Is had in bodies predisposed to Phthisis; but in a strong man and one who hath often before endured a Cough scot-free it will not be immediately to be feared."

“(2) But if the Cough grow daily worse and worse with plentiful and thick spittle, a languishing and pining of the whole body, loss of appetite, difficult breathing, thirst and fervent heat of the Blood be added, there is a great cause for suspicion that it is come at least to the first limits of a Phthisis (if not further).”

“(3) But if, beyond the state of this Distempe now described, plenty of Spittal and that discoloured shall be daily increased and all other things grow daily worse and worse, a defection of the whole Strength and a Hectic Feaver with a continual thirst, night sweats, an Hippocratical face, an utter decay of flesh almost to the dryness of a Skeleton, happen upon all these, then for the most part no place is left for Medicine but only a dreadful Prognostic.”

THOMAS SYDENHAM (1624-1689)

THOMAS SYDENHAM, the English Hippocrates, perhaps the most illustrious clinician of all time, deserves a great deal more space than I can give him in a book strictly designed to be a history of phthisis. His only contribution to this disease is found in a short note *De Phthisi*, found amongst his papers after his death and included in his works by Latham in his publication of 1848. And yet his adventurous life, full of dramatic incidents, no doubt, keeps one speculating—guessing, imagining, telling one's self stories—about a career of which the man himself said nothing, or next to nothing at all! Sydenham was a strictly *medical* writer; one whose whole tendency was to deal with what he could write of from his great store of knowledge. “For I have always thought,” says he, “that to have published, for the benefit of afflicted mortals, any certain method of subduing even the slightest disease was a matter of greater felicity than the riches of a Tantalus or a Croesus.” What did his past adventures, feats of arms, narrow escapes,

matter to him? He wrote no word of all this. Such things might happen to any man. They were not to be recorded of himself by "one of the Master Builders at this time in the Commonwealth of Learning" as Locke called him. We appreciate and applaud this reticence of the great Thomas Sydenham—but what would we not give for a few hints of what his life held for him while he was a simple captain of horse in Cromwell's army! We writers are like other children; we want *stories* of our great ones and how they bore themselves in their most human moments! Sydenham, however, had grown to man's estate ere he put pen to paper. He had put away childish things. We must be content with the knowledge that Boerhaave, that great Dutch physician, always removed his hat out of respect when he heard of Thomas Sydenham and called him, to his class, "Angliae lumen, Artis Phoebum, veram Hippocratici viri speciam." Sydenham was born at Wynford Eagle in Somerset in 1624 as the fourth son to William Sydenham, a gentleman of ancient lineage, by his wife, Mary, daughter of Sir John Jeffrey, Knight, of Catherston. Of his boyhood nothing is known but, in due course, he entered Magdalen Hall, Oxford, as a Fellow Commoner on April 20th, 1642. He cannot have spent more than about five or six months at the university when the Civil War broke out and he left to join his four brothers in the army of the Parliament in Dorsetshire. "The importance and zeal of his family procured him, at once, a commission as a Captain of Horse." (*Dict. Nat. Biography.*) He appears to have been at Exeter when the town was taken by the Royalists and he was, in all probability, a prisoner of war for nine or ten months afterwards. He must have been liberated or escaped for we are told of him, in the *Dictionary of National Biography*, as follows: "Colonel and Major Sydenham (in July, 1644) with their forces repulsed a Royalist attack on Dorchester from Wareham with great success and, in this engagement, Captain Sydenham, who had been a prisoner a long time with the Royalists in Exeter, behaved very bravely." His military service ceased, for the time being, in the Autumn of 1645, when the Royalist garrison in Dorset was finally reduced by Fairfax and Cromwell. He entered Wadham College

when Oxford was taken possession of by the Parliamentary Visitors and he was appointed one of the Visitor's delegates on September 30th, 1647. He was appointed by the Visitors to a Fellowship in All Soul's College on October 3rd, 1648, and on March 29th, 1649, was appointed Senior Bursar of the College. On the 14th of April of that year he was created Bachelor of Medicine by command of the Earl of Pembroke, Chancellor of the University, without having taken any degree in Arts. He must, however, have become a Master of Arts as he is so styled by the College of Physicians. "As Sydenham had only been resident six months at the University, his medical degree would have been rather the starting point than the goal of his medical studies." He seems to have been called up a second time as a captain of horse while holding the Fellowship at All Souls and to have been in Rich's army which sustained various actions against the Royalists of the Scottish army at that time. He may, in fact, have been wounded and "left in the Field among the dead" (Andrew Brown) but his movements seem to have been very uncertain. Dr. Lettsom's anecdote, which I give in full, was "copied from the fly leaf of the *Methodus Curandi Febris* in possession of Dr. Sherson's family for upwards of fifty years." (In the *Gentleman's Magazine* for August, 1801,) "Dr. Thomas Sydenham was an actor in the late Civil War and discharged the office of Captain. He being in his lodgings in London and going to bed at night with his clothes loosened, a mad drunken fellow, a soldier, likewise in the same lodging, entered his room with one hand gripping by the breast of his shirt, with the other discharged a loaded pistol into his bosom; yet, O strange! without a hurt to him. Most wonderful indeed, by such a narrow shield as the edge of the soldier's hand was his breast defended. For the admirable providence of God placed and fixed the tottering hand that gripped the shirt into that place and posture that the edge thereof and all the bones of the metacarpus were situated in a right line between the mouth of the pistol and his breast and so the bullet discharged, neither declining to the one side or the other but keeping its way through all the bones, in crushing them lost its force and fell at his feet. So wonderful a situation of the hand and more

wonderful course of the bullet by any industry or art never again imaginable! The soldier died soon after. Surely Providence does not bring forth such stupendous miracles but for some great and equivalent end!"

He had been for some time inclined to study medicine. In 1646, on his way to London in order to return thence to Oxford, he had chanced to meet with a Dr. Thomas Coxe, his brother's medical adviser, by whom he had been recommended to take up the profession. Later, "having obtained a medical degree with little or no knowledge" of the work, he had used his position at All Souls for the prosecution of his studies. On the authority of one Descault, a French surgeon, we know that he spent some time, as well, at Montpellier, then one of the most eminent medical schools in Europe, and we call to mind what Dr. Douglas has told us as to medical teaching for the Oxford degree at that time; but it remains uncertain whether this visit to Montpellier was before or after his grading as a B.M. on the order of the Earl of Pembroke; it was, probably some years after. It appears that he was granted by Cromwell, the sum of £600 as compensation for the losses sustained by the deaths of two of his brothers in the war, one killed in Scotland, the other in Ireland. This sum helped him towards the expenses of his marriage in 1655. As an Oxford B.M. he was admitted a Member of Pembroke College, Cambridge, where his son was then an undergraduate, in May, 1676, and took his M.D.—some thirty years after he had entered the profession of medicine! In the practice of his profession he was far from popular amongst the generality of doctors though he enjoyed the close friendship of such men as Boyle, Locke, Brady, Paman and Mapletoft. In 1663 he passed the examination of the Royal College of Physicians "for the licence to practice in Westminster and six miles round"—his only touch with the Royal College during his life. Crighton, in the *Encyclopaedia Britannica* says of him: "He seems to have been distrusted by the heads of the faculty because he was an innovator and something of a plain dealer." Rolleston¹ in his article of 1924, says that

¹ Rolleston, Sir H., Bart. "Tercentenary of Thomas Sydenham," *Brit. Med. J.*, Nov. 15th, 1924.

his view was that "Medicine depends on the observation of, not on hypotheses about disease." In a letter to Robert Boyle, one of the only samples of his own writing now extant, he says: "I have the happiness of curing my patients; at least of having it said of me that few miscarry under me; but I cannot brag of my correspondency with some others of my faculty. Though yet in taking fire at my attempts to reduce practice to a greater easiness, plaineness, and in the mean time letting the mountebank at Charing Cross pass unrailed at, they contradict themselves and would make the world believe I may prove more considerable than they would have me." His first great publication, *Methodus Curandi Febris*, was given to the public in 1666; about twenty years after he had started to study medicine. A second edition, with a chapter on plague, appeared in 1668, and a third, entitled on this occasion *Observationes Medicae*, in 1676. There was also the *Tractatus de Podagra* of 1683, the *Schedula Monitoria de Novae Febris Ingressu* in 1686 and twenty copies of his *Processus Integri*, after his death, in 1692. He died "in an acute paroxysm of gout" in December, 1689, and was buried in the church of St. James's, Piccadilly.

His work on consumption of the lungs, *De Phthisi*, is very short and, perhaps not very important except for one thing, his insistence on riding as a valuable treatment. This invitation to take open-air exercise, with a horse under the patient to do most of the exertion, deserves to be recorded as a great step in therapy and both before and afterwards was advocated by other experts. The tendency of Sydenham to give short and often incorrect *theoretical* concepts and to describe the *best practical* therapeutic steps to be taken is characteristic of the man. He was never, it appears, very strong on theory but his practice was invariably worth the most thorough consideration. "To this disease" he says, "are most incident men and women from puberty to the state of life, i.e., from 15 to 25, after which the diseases resulting in consumptions are the effects of some other causes than those which produce this." Was he, here, drawing an inspired contrast between the young adult type, striking the patient severely from the first and easily recognizable as phthisis, and the more chronic types, coming



THOMAS SYDENHAM (1624-1689)

on in a manner only recognizable at first as "winter cough" or severe catarrh, but developing into an actual tuberculosis later? Now, with the X-ray to guide us, these types tend to fuse and commingle, but the contrast may be sharp nevertheless. He goes on: "By a peculiar infelicity of our air none are more subject to it than the inhabitants of London." This was probably true at that time, the country people tending to come up more and more to the capital from their rural isolation and to be exposed for the first time, perhaps, to the intimate contact associated with a city population. "The kinds of Phthisis differ altogether in their causes and consequently require different cures." Some of them were, no doubt, curable, not being true cases of consumption at all, and some genuine types may have been the mild cases that tend to a natural amelioration, but we must all agree that different manifestations of consumption require different kinds of *treatment*, whether they ever reach a permanent cure or not.

He seems to think that the noxious influences which lead to a phthisis are to be found in the circulating blood which "discharges them upon the lungs either immediately by the branches of the *vena arteriosa* or first by the arteries upon the spongy parts and the fauces and thence by the *aspera arteria*¹ and so into the lungs." The lungs "are rendered unfit to assimilate the blood that is brought to them for their nourishment which is therefore laid up and constitutes the greater part of the matter which is expectorated by cough." Sydenham is impressed by the fact that some coughs disappear in due course while others continue and ultimately become, or rather manifest themselves as, true phthisis. "For a little before the winter solstice, upon the first approach of some bitter cold, almost everybody coughs, viz., the transpiration being suddenly checked. . . . Some by ill management keep their coughs so long that their lungs are at length much debilitated by the innumerable succussions that are continually made by the act of coughing. . . . In this case other undigested humours from other parts are sent into the lungs as being the weakest part. . . . But hence proceeds in process of time that extravasated matter

¹ Trachea.

is collected up and down in the vesiculae of the lungs which at length has little bags or cystides giving upon it, the matter contained in them turning by degrees into pus. . . . The lungs being thus repleated with pus, from them flow purulent streams into the blood which cause a sort of putrid fever whose access is towards night and its solution towards morning by a profuse and weakening sweat. Lastly towards the completing of this tragedy comes a *diarrhoea colliquitiva* . . . and then death is at hand. . . . When this cough has continued long, then the patient begins to sweat at night which is the first sign of a consumption coming on, and after this he begins to have a hectic heat . . . though the patient has, all the while, a serenity of mind and flatters himself with an opinion of recovery which is usual in this disease even to the very last."

We see in this passage, I think, the poor theory in common use at that time, without any attempt to better it; and, at the same moment, a very correct and true observation as to the condition of the actual patient and his touching "*spes phthisica*," to which he clings in spite of night sweats, *diarrhoea colliquitiva* and everything! After this he tries his hand at a little of his favourite epidemiology. "The frequency of consumptions in London is for that we live here in a perpetual mist, the sun not being powerful enough to dissipate the clouds, and with this mist are mixed the fumes that arise from the several trades managed here but especially the sulphur and fumes of sea-coals with which the air is repleated and these, being sucked into our lungs and insinuating into the blood itself, give occasion for a cough." This he regards as the first sort of consumption. At least it is a true picture of the kind of atmosphere that Londoners had to breathe in those days making a sharp contrast with the balmy air of the still unspoilt countryside. "The second sort of consumption is laid in quite contrary season, viz., in the beginning of the summer; for about that time a spitting of blood happens often to such young men whose blood is weak but hot and sharp after violent exercise or a debauch of drinking. . . . A third sort of phthisis happens in the end of a fever, when the febrile matter is discharged upon the lungs and so, in the place of the essential fever, there succeeds a hectic . . .

and, not very long after, a *diarrhoea lethalis*, for they soon die of this sort of phthisis." The consumption of the young adult must indeed have been very acute in Sydenham's day. He also speaks of the phthisis of children. "Consumptions are also laid in children after measles and chin-cough." It was evidently the same then as now. Measles and whooping cough still bring into prominence the essential infection of phthisis!

After this he proceeds to speak of his treatments and, especially, of the virtues of syrup de meconio in *aqua lactis* by which method, he says, "I have cured many." And here he suddenly gives us his great remedy! "And though riding hath done well in hypochondrial and other distempers, yet it does better in a phthisis than in any other case; for by repeated succussions of the lower belly (in which are situated most of the separatory glandular organs) those are put upon the performing of their several functions by having the natural heat excited and the blood is by this means depurated and (as it were) churned over anew. . . . I am sure that if any physician had a remedy for the curing of a phthisis of equal force with this of riding, he might easily get what he pleased; in a word I put very many upon this exercise in order to cure consumption and I can truly say I have missed the cure of a very few." Riding! It is not an exercise that we should recommend to everyone nowadays. But let us remember that it was the one form of transport in Sydenham's time. Everybody could ride and there were always horses available. The great thing was to get consumptive patients that were still fit for it *out of the house and into the air and sunlight*, with a little gentle exercise as well. Riding would hardly be "gentle exercise" for one who had not the habit of riding and the effort to find a "hireling" of a suitable kind would itself be a strain and a bore. But the essential thing, exercise in the open air, is still one of the best remedies for those who are well enough to enjoy it—albeit the temperature may now-a-days perhaps be taken just after, as is done in most sanatoria. Undoubtedly Sydenham was right.

RICHARD MORTON (1637-1698)

RICHARD MORTON was born in the days of Charles I, and his life did not end until William III had been ten years on the throne of England. His span included some of the most stirring years in the history of our country; the transition from the romantic and dangerous times of the Stuarts to the red days of the Revolution, the Protectorate, the Restoration and the florid period of relapse from a too strict to a too joyous mode of life; the passing from religious turmoil between High Church and Dissent to an extremely ritualistic attitude; almost to Roman Catholicism in fact, and finally to the establishment of toleration under the imported indifference of William and Mary. All these changes must have had a marked influence on an extremely intelligent youth making his way upwards to maturity and to be a minister of the Church filled with the spirit of Protestant Dissent. Of Morton's final rejection by or of the Church and his fortunate transfer to medicine we shall tell as thoroughly as the materials in the literature allow us in the following pages.

Morton was the son of the Rev. Robert Morton who had taken up the living at Bewdley Chapel in Worcestershire in 1635. Richard was born in 1637, two years later. I suppose that he grew up, as other boys do, with a strong proclivity to country sports and a mixture of admiration and awe for his father's religious character, but I can find no account of his boyhood anywhere. At any rate he gradually came to a practical adoption of religion as the best thing to follow in life and, after matriculating at Magdalen Hall, Oxford, on March 17, 1653, and passing thence to New College, where he obtained his B.A. degree on January 30, 1656, became chaplain to the college. Here he stayed for three years and then took his M.A., proceeding as chaplain to the family of William Foley of Brentwood in Staffordshire and being appointed vicar to the parish of Kinver in the same county. All this time, Cromwell

had been "Protector," ensuring to the people of England a relatively quiet life. I infer, too, that Morton's life, for the next three years, was the usual uneventful one of a country clergyman, but events of an unusual importance were taking place outside the sphere of his ministrations and were destined to face him with an alternative in the near future.

In 1660 the Restoration occurred; and with it the revival of High Church principles. Trevelyan's *History of England* (London, Longmans, Green and Co. Ltd., 39, Paternoster Row, 1926) speaks of the Act of Uniformity, 1662, as follows:

"The rest of what the Dissenters were now to suffer may be traced to the Parliamentary fines on Malignants and to the executions of Laud and Charles I. It was not merely vengeance; the Clarendon Code was also a measure of police against the revival of the Roundhead party. The Act of Uniformity in 1662 restored the Prayer Book and turned adrift without compensation 2000 clergy who could not assert their 'unfeigned consent and assent' to everything the book contained. The Conventicle Act of two years later made prison and transportation the lot of those caught in acts of dissenting worship. Laud's religion triumphed, not through the royal power nor through the clerical jurisdiction and authority which he had striven to restore, but through the action of the Parliament of Squires whose right to pronounce upon religion he and Charles I had died rather than acknowledge. . . . But in the interim between the Restoration and the Revolution, the nation was torn and tortured by the active persecution of so large a body as the Protestant Dissenters. . . . But many of the middle and lower classes, in the days of Baxter, Bunyan and George Fox, suffered ruin and imprisonment rather than give over the attendance at services which Parliament had made illegal."

Morton was one of those Protestant Dissenters who found it impossible to subscribe to the Act of Uniformity and, as a result, he was pitch-forked out of his "living" and forbidden to practise his religious avocation again. What did he do in this period of discouragement and disgust at the works of his

fellows? He appears to have adopted that equally important profession, medicine, as his best way of serving his generation, and to have devoted his time to learning the intricacies of his new calling somewhere or other; probably at Leyden as well as at his own University of Oxford. There was at that time a vigorous School of Anatomy at the University and it is probable that he took advantage of it to master that part of his work though there is, as far as I am able to ascertain, no record of his having done so or indeed of a definite Medical School at Oxford at that time. He may also have transferred himself to Holland, and have studied at the University of Leyden; a plan which commended itself to many at that time. Here he would have had an opportunity of making the acquaintance of the Prince of Orange; doubtless on the look-out for Englishmen of the right convictions, as he thought, for his own possible future in our country. There is, at least, the evidence of Morton's career to support this idea for, on the nomination of the Prince of Orange, he was created M.D. of Oxford on the 20th of December, 1670, and afterwards settled in London to practise his profession. Now how, unless indeed he had actually studied his medical subjects in Holland, could the son of a country clergyman, himself one who had professed a similar role, have made the acquaintance of the Prince of Orange and not only made his acquaintance but succeeded in getting his name accepted for nomination? There appears to be no doubt that the Prince did visit Oxford and managed to get degrees conferred on a few members of his own train—and probably some others as well. A reference to the Bodleian Library, elicits the facts which are said to be discoverable in Wood's *Life and Times* (Ed. A. Clark) Vol. II, pp. 206-211, but which I have as yet, been unable to obtain. To the Bodleian I am also indebted for the following letter preparing the university for the royal visit.

Mr Vice Chancellor and Gentlemen,

At the solemn Reception of the Prince of Orange who vouchsafes to honour the University with his presence, you may very well believe I have been very much importuned to recommend divers persons to you for degrees; but

considering how much the honour and interest of the University is prejudiced by such irregular proceedings, I have (as you, Mr. Vice Chancellor can witness me) strongly resisted such Solicitations; however I have been prevayled with some few whom I could not refuse without disobliging some honourable persons whose favour and protection the University may have need of.

I shall not need to desire you to pay all due honour and Respect to his Highness and those who attend him, especially those Hon^{ble} persons of his owne Trayne; and by your Diplomas to conferr degrees upon his Chaplins or any other learned persons abroad whom he shall recommend to you; whatever you shall do of this nature I shall approve of and am

Mr Vice Chancellor and Gentelemen,

Your very affectionate Servant,

Ormond.

Clarendon House

17: Decemb: 1670.

(From the Register of Convocation. 1659-1671. Univ. Arch. T. a. 27).

The Duke of Ormond was, at that time the Chancellor of the University and we can clearly appreciate how much he had been bothered by persons, probably very like Morton, who saw in this visit a chance of getting their degrees conferred in spite of the prevailing opinions. Morton, at any rate, was successful, and it must have been with considerable mortification that his fellows of a different way of thought saw him made a Doctor of Medicine of the University of Oxford.

He now settled in London to practise his profession and seems to have done very well; well enough to endeavour to enter the Royal College of Physicians. He was admitted a candidate for the College on the twentieth of March 1675/76, and became a Fellow on the twenty-third of December, 1679. In 1680 he was incorporated at Cambridge on his Doctor's degree. There is still evidence available that he was marked down for every slight that might be possible owing to his religious opinions; this was manifest even in his career as a

doctor. Morton was one of the four Fellows of the College of Physicians whose names were omitted from the Charter of James II in 1686. But better times were in store for him. In 1688 occurred the Revolution which was to see James II a fugitive and to set William III, the Prince of Orange, and his wife, Mary, on the throne and to bring hope to many unfortunates whose existence or whose interests had been so gravely compromised by their Protestant convictions. Morton was duly installed in his position as a Fellow of the College of Physicians in 1689 as a result, we cannot doubt, of the raising of the ban on those who had held on to their religious convictions during the efforts of James II to introduce or, at least, to encourage Roman Catholicism. He became a censor and held this office during 1690, 1691 and 1692, and was one of the Physicians in Ordinary to the King. He resided, we are told in the *Dictionary of National Biography* already quoted, in Grey Friar's Court, Newgate Street, and carried on an extensive practice, seeing and, doubtless, benefiting many sufferers from phthisis, both those whose wasting was due to general disease and those, more numerous, whose phthisis was due, as we now know, to the tubercle bacillus. Thus he lived and worked for the greater part of ten years from the Revolution which had meant so much to him, and finally died and was buried, we are told, on August 30th, 1698. Baxter says of him that he was "a man of great gravity, calmness, sound principles, of no faction, an excellent preacher, of an upright life." We should judge him so from his work which is that of a calm and judicious writer. He treats of consumption and its post-mortem appearances as well as its clinical characters with an integrity which is very much to the credit of his sound understanding and detachment from many of the ancient dogmas so prevalent in his times. He seems to have had at least three children, a son, Robert, and two daughters, Sarah born in 1685, and Marcia in 1689. Robert was destined to follow his father in the medical profession and was, in due course, a Fellow of the College of Physicians. Of his daughters we can find no trace except their names recorded in the *Dictionary of National Biography*. Morton's is one of those lives which are only to be thought of



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in relation to his career and his work. How much of interest must lie behind these brief and practical records! But the story of his life is long forgotten and only the story of his life's work remains in his prodigious effort to comprehend phthisis and consumption without the aid of that explanation and simplification which was to come only in our own time or shortly before it in the observations of Villemin and the discoveries of Robert Koch.

Let us, as we cannot come by the detailed knowledge that would be necessary to write his life, examine his great work, *Phthisiologia* (seu Exercitationes de Phthisi, London 1688, and in later editions) and endeavour to understand his point of view and his convictions about a disease which, although now well understood from the aetiological side, still keeps us guessing as to how to diagnose it before it has made sufficient progress to be entirely recognizable and, when diagnosed, how best to treat it in the light of our present knowledge.

Morton, taking the word phthisis or consumption to mean exactly what it does mean, *a wasting of the body from any cause*, divides his work into three books, the first dealing with wasting in general, the second with a wasting from a consumption of the lungs, and the third with a symptomatic consumption, a consumption of the lungs caused by and depending on some other preceding disease. When we think how he was straightened for diagnostic facilities and impeded by the want of what we now enjoy in the way of confirmatory measures, this division seems a very sound way of approaching the subject. It was, of course, impossible for him to exclude from it errors of one kind or another. Wasting diseases, for instance without fever, cough or shortness of breath, might still be due to the tubercle bacillus though all unrecognized for tuberculosis! "Fear, Grief and Anger," too, are hardly to be accepted as causes of an "original" consumption of the lungs nowadays, although they may play a part in the psychology of chronic phthisis now so much studied, but we can easily follow his idea in including them. We can readily admit, too, that there may "remain and lurk in the Blood some indelible Impressions and the seeds of a

Consumption that will afterwards follow," as he correctly says of his "symptomatical" phthises. Is not "allergy" an impression on the blood? And we need not quarrel with the expression "Seeds of Consumption" when we recall that tubercle bacilli, freed from foci that otherwise might have remained latent, are often liberated by antecedent diseases such as measles, whooping cough and other such crises.

Let us turn, now, to the consideration of some of the cases recorded by Morton in his three headings:

In the first, or the type of phthisis without a cough, fever or shortness of breath, he treats of "consumptions" from "some evacuation," from "bleeding" from haemorrhoids, from the throat by hawking, from the vessels of the womb, from the urine and from the lungs; in the latter case, however, he does not refer to either haemoptysis or to the vast bleedings which sometimes take place in tuberculosis, but to other occasional haemorrhages; those following bronchiectasis, perhaps, though he did not, of course recognize this condition. He goes on to consider other causes of wasting without lung symptoms; "consumption proceeding from apostemes and large ulcers" for instance, in one case of which he describes a patient who had a swelling from which, when opened, came out first water and then pus and, finally, "small bags filled with water," at least five hundred at one evacuation! It has been my lot to deal with such a case. It was an echynococcal cyst of the right lung. In Morton's instance, the aperture was deliberately kept open for a year and a half "after which she was plainly free from her state of marasmus" and got quite well again. This case indicates clearly the type for which Morton was seeking in his first varieties of wasting; plainly *not* a tuberculous type. And yet he sometimes runs into excusable error. He describes, for instance, a consumption "happening to nurses giving suck beyond what their strength will allow" and cites a patient who "at length falling into a consumption of the lungs with a cough and shortness of breath and hectic Fever, she died plainly choak'd." Here he describes the symptoms correctly but attributes them wrongly but naturally to the long lactation instead of to the tubercle bacilli to which

the lactation gave the opportunity. He gives a first-rate account of yet another case, a consumption "proceeding from a Dropsie," which happened in a child of two years old with a large collection of fluid in the abdomen. At the autopsy the nature of the disease is obvious to our eyes though, to a certain extent, hidden from his. I use the words "to a certain extent" advisably for Morton made as good an attempt as was possible at the time to comprehend the case from the post-mortem point of view. "But when we opened the Body" he says, "after he was dead, we found, for all the difficulty of Breathing and the long coughs he had had, the lungs themselves sound, without any Distemper, but only that, in the hinder part the Wind-pipe, there appeared a great many glands and those pretty large and hard, which made a considerable pressure upon the chyle-duct itself almost in that part where it arrives in the Subclavian Vein; and they were of so great a weight and bulk that it seemed very difficult, if not plainly impossible, for the chyle to pass into the Blood by reason of the pressure they had made upon the Duct which had straightened it as if it had been tied with string." To a man looking for the kind of tuberculosis of the lung to which he was well accustomed, the obviously healthy state of the lungs of this child must have seemed evident, except perhaps, for a primary focus which was not likely to be noticed. The big tracheo-bronchial glands, however, and especially that pressing on the thoracic duct, are beautifully indicated, and the train of symptoms correctly described. "From what I have said" he adds, "it is plain almost to demonstration, first, that these Tubercles or tumified glands of the Lungs did at first proceed from the ill method of curing the Inflammation in his Lungs, that is for want of timely Bleeding and Expectoration." This remark we may pass over with a shrug as we have not yet a cure for the primary complex. But he goes on: "Secondly, that as the troublesome and dry cough was caused by the condition of the Vessels of the Lung which convey the Air, by these swellings, so the difficulty of Breathing proceeded from the pressure of the extra-vascular chyle below the midriff." He might better have attributed the difficulty of breathing to the same cause discovered by him as the

reason for the cough. "Thirdly, that this Consumption was not a true Consumption of the Lungs; because, though there were Tubercles or swellings, yet they were not Apostemes nor Ulcerated." As I have said above he was not in a position to know about the primary complex and its occurrence in healthy lungs. "Fourthly that the Dropsie did proceed from a rupture of the small Lacteal Vessels in the Belly; and, Lastly, that the rupture of the small Lacteal Vessels was caused by that continued pressure which the swellings in the Lungs made upon the upper part of the Chyle-duct." This conclusion seems entirely justified by the facts of the case and, moreover, redounds, I think, to the credit of a pathologist working in the uncertain light of the time.

These two instances are quoted to show how one depending on the absence of symptoms, at first, pointing to lung trouble might be deceived as to the nature of cases and regard certain of them as not tuberculous although the evidence is now quite clear that they were so.

But, this is not to deny that Morton was right in most of his instances and that the consumptions caused by salivation, etc., were, as he thought, of a general nature and not tuberculosis. It would be difficult, however, to say so much for all his types. We refer particularly to the consumptions "caused by Profuse Sweats" and those brought about "by many and large Scrophulous Kernals preternaturally seated in the mesentery . . . by which the passage of the nutritious Liquor, which is separated in the Intestines and taken in by the mouths of the Lacteal Veins into the Mass of the Blood is either totally or in part hindered." Here, while applauding the very rational way in which the essential lesions were discovered and the symptoms rightly attributed to them, we must again invoke subsequently acquired knowledge to attribute them to their true source. The fact is that it is very difficult to differentiate the majority of cases of wasting and to attribute them to diseases other than the tubercle bacillus without having a more thorough grip on the causation of tuberculosis than was possible at that time.

In passing to the Second Book, that which treats "of an

Original Consumption of the Lungs," we pass over, at this stage, his ideas as to the causation of phthisis but we shall return to them later. We turn, rather, to the signs, either "Prognostic, Diagnostic or Pathognomonic" which served him in his examination of cases. Under "Prognostic" he repeats about sixteen of the "causes"; "A troublesome and chronic Heat," etc.; all recognized nowadays as the results, not the causes, of tuberculous disease of the lungs. Morton enjoins, however, certain rules that guided him in the prevention of subsequent disease in those who seemed likely to develop it and which we might very well lay to heart today. "Therefore in the preventing of a Consumption (which is very much easier than the cure of it)" he writes, the great thing is "to take all possible care that no error be committed in those six things that we call 'non-natural.' For in this so slippery a state of Health they are wont, upon every little occasion to fall headlong, into a Fatal Consumption. As, for example, they ought to be prudent in choosing their Meat and Drink that the Chylous may be made to abound with good juice and that the Nourishment may create very little trouble to Nature in digesting and dispensing with it. Let them also heed not to eat too much food, though it be such as affords a good Juice, as also that they do not drink too much Wine or Strong Liquors. *Secondly*, let them sleep in the fore-part of the Night: but let them avoid sleeping in the day time, yea, and sleeping too long in the morning; because such sleep is wont to retain and heap up a great load of Humours in the Habit of the Body. *Thirdly*, let them use every day moderate exercise and rubbing for a good while together, to fetch out the dispirited Humours from the Habit of the Body by the pores of the Skin. *Fourthly*, let them strictly avoid all strong Purges; for as much as they not only weaken Nature but also, by putting the Blood into too great a motion with their sharp Particles, they make it grow acrid and hot and bring it into a more serious and colliquitave state; upon which a Catarrh and a Consumption are wont to follow. *Fifthly*, let the patient by all Lawful ways industrially lay aside care, melancholy and all poring of his Thoughts as much as ever he can and endeavour to be cheerful. For I have observed that

a Consumption of the Lungs has had its origin from long and grievous Passions of the Mind. *Sixthly*, let the patient enjoy an open, fresh and kindly Air and such as is free from the Smoke of Coals, which may not only cherish the Animal Spirits and comfort the nervous Parts, and, consequently restore the weak Appetite, but likewise procure quiet (at least in some measure) to the Lungs. But there must be great care taken that he does not get a new Cold. For the Body being in such a manner filled with a load of Humours, every new Catarrh or Cold tends to a Consumption and from hence come all our Sorrows." What *comfortable* advice! And all of it sound! For the man who has had warnings that *something* may be wrong with him, that the rightness of his adjustments is not quite so right as it used to be, that he cannot respond to the exaggerated claims of these exacting times with a sufficient effort, we cannot imagine better advice. Apart from the calling up of the Humours it might well be given to a possible phthisic today! And especially we may appreciate the sentence under "*sixthly*" where he enjoins fresh air and other things which may "procure quiet (at least in some measure) to the lungs." Is not that what we have been seeking, at every stage of the disease, by "absolute rest," "artificial pneumothorax," "thoracoplasty" and all the measures that we now recommend? It is significant that Morton, too, recommended this "quiet" which we still seek; often in vain!

Among the "Procatartic causes or those which give the first occasion to the Disease," he mentions those that lead to a stoppage of usually got-rid-of discharges as amenorrhoea, and repeats several of the "six non-naturals" already given; "A foggy and thick air and that which is filled with the smoke of Coals" for instance. He goes on, however, to describe others more important and of more immediate concern to us even at the present day; "An Hereditary Disposition from the Parents," for instance, which is by some regarded as of great importance still. This opinion was hardly to be avoided in Morton's time as it was often evident in practice that the children of contaminated parents got, in their turn, the same disease; a fact which could only be attributed to heredity when the bacterial nature of

the disease was not known. We consider, now-a-days, that the occurrence of disease in the offspring, is due to intimate contact of the infants and children with infected parents. While admitting that there is something in the hereditary nature of tuberculosis we maintain that it is this early and intimate contact that leads to the disease being more prevalent in the children of the tuberculous than in the children of persons incapable of spreading infection, and we are able to cite the large number of "positive" tuberculin reactions that arise among these "contacts" as compared with others. We affirm, in fact, that the prevalence of manifest disease, and, much more, the high incidence of the evidence of *infection* produced by the tuberculin test, amongst the children of the tuberculous as compared to the children of the unaffected, is *proof* that bacterial contamination, not heredity, is the cause of tuberculosis. Take the following, for example: "Mr. Hunt, a citizen of London, lived almost from his youth to the seventieth year in a consumptive State, doing his business well enough by taking care." From the age of sixty, however, he began to go down hill slowly, but "was easily freed" from his symptoms "by the advice of a Physician." He had three sons who all lived until they were about thirty "about which time they were all, one after another, seized by the same right of inheritance with a consumption occasioned by Passions of the Mind and the drinking of Spirituous Liquors" whereby "the Distemper . . . carried them all off before the emaciated old man died." The "right of inheritance" was, of course, the intimate character of their association with their aged father and their inspiration of his copious output of tubercle bacilli. He probably broke down into a source of large doses of germs at about the same time as he began to go slowly down hill. One of the three was treated by Morton and appeared to get better but relapsed the next summer and died. His widow, also, got a bad touch of the prevailing consumption but, under Morton's treatment, ultimately recovered. She may be noted, we think, as an instance of a bed-fellow being contaminated with the same disease.

Another cause he finds in "An ill formation of the Breast,"

either natural or accidental, due to a "crookedness or distortion" of the thorax; a feature which we recognize as either caused by or causing tuberculosis today. But the next is the most interesting of all: "This Disease (as I have observed by frequent Experience), like a Contagious Fever, does infect those that lie with the Sick Person with a certain taint." Even then and with only the crudest notion as to what infection really meant, Morton was unable to envisage the frequent consumptions arising in the bed-fellows of the tuberculous without being seized with the notion of the transmission of "a certain taint"; he describes several causes of phthisis arising in this way in his notes.

He cites, too, as causes of pulmonary phthisis, "Chalky Stones that are preternaturally bred in the Lungs," obviously the results and not the cause of phthisis, and adds all the maladies which are followed by his "symptomatic" consumption. In his third chapter Morton treats "Of the Diagnostic and Pathognomonic Signs of the beginning of a Pulmonary Consumption." These are "either such as discover it when it is but begun or when it is once confirmed and deplorable." He quite recognizes that the first signs available to him—he was, of course, without either the percussion to be later introduced by Auenbrugger or the stethoscope of Laënnec—were either the earliest signs of tuberculosis or, often, the signs of advanced disease. "The Pathognomonic Signs of the beginning of a consumption of the Lungs are, first, a Cough, which one may, and that by very certain signs, distinguish from a simple Catarrh, how violent so ever and Chronical it may be, because, firstly speaking, the Part affected or seat and source of both Distempers are not the same." "A Consumptive Cough proceeds from a *Glandulous Swelling or Tubercle of the Lungs themselves . . . On the contrary, a Simple Catarrh owes its Origin from a distillation of rheum cut out, as it were, in continual drops by the Uvula and Almonds and the other Glands seated in the upper part of the Wind-pipe, yea, and by all the Glandulous Coat of the Wind-pipe itself.*"

He speaks of the early cough of the tuberculous as something to be distinguished from a catarrhal cough by its characters.

“For as it is dry and without any expectoration, so it is not great nor the Fits long and is rather made up of its own accord, to relieve the Oppression of the Lungs than excited by a violent tickling or accompanied by great straining which usually accompanies a Catarrhus and fierce Cough.” It is quite evident that Morton was a close observer and that he had seen the essential differences between the cough of early tuberculosis and the cough of an ordinary catarrh, though he freely admits in his treatise that instances may occur in which these coughs are indistinguishable. There is one passage with reference to tuberculosis of the lungs which seems to me to have been almost inspired, so much is it in conformity with modern views. “Yea, when I consider with myself how often in one year there is cause enough ministered for producing these Swellings, even to those that are wont to observe the strictest Rules of Living, I cannot sufficiently admire that any one, at least after he comes to the Flower of his Youth, can die without a touch of Consumption.”

He proceeds to record the other signs of tuberculosis of the lungs: “As there is always a want of Appetite and a Thirstiness accompanying a Consumptive Cough, so likewise a vomiting after eating uses commonly to follow it.” He must have met a great many rather advanced cases in his search for early ones. “Likewise a hoarse or shrill or squeaky voice joined with a Cough, if it be frequent, much more if it be continual and chronic, shows it plainly to be a Pulmonary and Consumptive Cough.”

“The second Pathognomonic Sign of the beginning of a Consumption is a Fever . . . The third is a Wasting which, in the beginning of a Consumption is very slow . . . until from an Inflammation of the Tissues . . . a putrid Fever comes to be joined with the Hectic . . . so that within a few weeks the Patient has a Hippocratic Face.”

He goes on to describe the type of fever: “A Tertian and that at a certain time of day beginning with a chilliness and coldness, proceeding with a great heat, and so at last ending in profuse and colliquative Night Sweats . . . at which time the Patient sleeps quietly, breaths not so short as before and

plentifully coughs up the Concocted Phlegm without any difficulty or pain. But by how much the Fever is more acute so much the shorter it is; and by how much the more moderate so much the longer it lasts." It is hard to imagine how he assessed the fever. Thermometers were then almost unknown. Although Sanctorius Sanctorius, in Padua (1561-1636) had already invented a "clinical thermometer" which may have penetrated to England. He probably relied upon the *feeling* of the patient's skin at which he must have become very expert; but perhaps he used a thermometer. He goes on to describe the other characteristics of this disease: "From whence there wont to arise two new Symptoms and they are very troublesome ones; to wit, a great heat about the Tonsils and the parts that serve for swallowing." He refers no doubt, to laryngeal ulcerations for he proceeds: "Whereby it comes to pass that the Patient can scarce swallow anything but with grievous pain." And the other is "Marasmus with a Hippocratic Face."

Finally with regard to the pulse: "In the beginning of a Consumption, whilst there is no other Fever but a Hectic, as the preternatural heat is contained and moderate, so likewise the Pulse is somewhat quicker than it ought be according to the degree of the Fever . . . But as soon as ever the Peripneumonic Fever seizes them, the Pulse, as it uses to happen in other Peripneumonias, is not only quick but also hard and strong; yea, rises up more in one plain than another so as to feel somewhat like a Saw when one feels it with several Fingers together . . . Though sometimes, by reason of the violence of the spasmodic pain caused by the Inflammation of the Tubercles, it is no rare thing to observe a weak Pulse."

These tuberculous symptoms are sometimes very chronic and sometimes so acute as to be very rapidly fatal. To illustrate the slow or "chronical" type he describes the autopsy on a Mr. Davison as follows: "For all the Swelled Glands are not disposed to an Inflammation at the same instant of time, as it was evidently apparent to me in the Body of Mr. Davison who died of a Fatal though a Chronical Consumption, when we came to open it. For in the Lungs of the dead Body we found at the same time some Tubercles that were turned to Apostemes

and others that were inflamed and, lastly, some that were crude and unripe."

Let us now pass on to "Book the Third" wherein Morton treats "Of a Symptomatical Consumption of the Lungs," a consumption "caused by and depending upon some other preceding Diseases . . . And indeed this kind of Consumption (as far as I have been able to observe) is the most common of all others." He begins by cataloguing the various diseases that may be followed by a consumption—"Crapulous and Intermitting Fevers, Small Pox, measles, scarlet fevers, a Pleurisie, Peripneumony, melancholy, Diabetes, "Dropsie of the Chamber Pot," as he calls it, the King's Evil, etc."

"So I think 'tis no wonder if those that have the King's Evil who are frequently subject to glandulous swellings in other parts are likewise many times affected with such kind of Tubercles, even in the Lungs themselves, the substance of which is naturally spongy and apt to receive the Serous Particles of the Blood that are plentifully separated by their continual agitation." But he goes on to describe what happens, not only in cases of the King's Evil, but in all consumptions following on other diseases: "These Tubercles of the Lungs (as Scrophulous swellings of other parts) are: Either crude and phlegmatic and so no ways disposed to an Inflammation, or else more hot, which will be seized with an Inflammation sooner or later . . . For sometimes, to wit, when the Matter is concocted and hardened into a chalky or steatomatous substance or into the substance of a *meliceris*, which it commonly is, the inflammation and exulceration that proceeds from thence are not only some considerable time before they happen, but likewise, when they do, they are very slow and almost insensible : . . . But whenever these Tubercles are of a hotter nature and therefore more subject to a quick Inflammation and Exulceration this scrofulous Consumption is very acute and terminates in a few months." Thus he draws a sharp distinction between the chronic and relatively mild consumption in persons who "live, though they are crazy and sickly from their childhood 'til they are old with a Cough which is almost continual . . . but yet without any sensible

Fever . . . These people may be preserved from any dangerous and Fatal effects even without Physic''; and the much more acute and usually fatal cases that often occur after other diseases and, we would add, especially after the diseases of childhood, measles, whooping cough, etc.

To give point to this distinction between the chronic and the acute we here describe another autopsy, that of the only son of the Mr. Davison whose post-mortem we referred to as showing the signs of a chronic case. This young patient had, from his birth, "a purulent scab scattered up and down his whole Body" which he finally got rid of "with I know not what Repercussive Plaisters and Oyntments that he made by the advice of some Old Woman." This cure of the "purulent scab," instead of being welcomed as something at least to the good in a poor kind of case, was thought by Morton to be a direct cause of the subsequent phthisis. "Whereupon, presently, he was troubled with a dry Cough all the Summer from Tubercle of the Lungs occasioned by a driving in of his Scab." The patient went rapidly from one stage to another until he died "through impatience and his averseness to medicines," as Morton puts it; the post-mortem took place shortly after. "In the Body, when 'twas opened, we found all the lobes of the Lungs here and there bespattered with Tubercles of a various magnitude; some that were small and newly bred; others that were pretty large though they were crude; but some that were inflamed and exulcerated, containing in them a purulent matter that was of the consistency of Honey. This I took to be scrophulous Consumption of the second sort; that is, Hot and active."

William Bulloch, in his Horace Dobell Lecture delivered before the Royal College of Physicians in 1911, attacked Sylvius for originating and Morton for following a line of thought which, he said, "hampered advance for more than a hundred years. . . . As is well known," he said, "Sylvius derived tubercles from the degeneration of small invisible glands seated in the substance of the lung, and he considered that the tuberculous change was analogous to the scrophulous change so

common in the glands of the neck and mesentery. In his learned *Phthisiologia*, Richard Morton, Surgeon of Newgate Street, could come to no other conclusion." But was the theory which Morton advocated—perhaps referable to the work of Sylvius—as regards the involvement of glands in or of the lung so very wrong? Ever since the days of Ghon, and indeed, before it, we have recognized the part that the glands, both tracheo-bronchial and pulmonary, play in receiving and maintaining the tubercle bacilli from the primary focus in the lung; and does anyone now doubt the tuberculous nature of the "scrofulous change so common in the glands of the neck and mesentery"? And, further back, in primary tuberculosis at any rate, it would be very hard to exculpate the very small and hardly noticeable but still present lymphoid collections at or near the divisions of the bronchioles from being the most early infected parts of the lung; primary tubercles are at least approximated to the glandular and lymphatic circulation. Morton, in his utterance as to the origin of tubercles was guessing—and perhaps guessing wrong—as were Dessault and William Stark, quoted by Bulloch as rectifying his errors; but could any of them have guessed that two persons, Villemin and Koch, should afterwards, in the fullness of time and the rising tide of research, prove that tubercles were really tissue reactions to specific bacteria? To me it seems that the marked stress laid by Morton on the *glands* of the thorax as being the especial seats of tuberculosis and his stress on the similar formations in the mesentery is one of the signs of genius guided by the only correct method of observation, the examination of the body after death. What matter if he mistook the sites of secondary tubercles for glandular sites also; he stressed the *glands* as the places where tuberculosis was invariably present; the tracheo-bronchial, the broncho-pulmonary and other thoracic glands as the most important sites of its accumulation; a wonderful generalization which has at last come to be recognized as of basic importance in the pathology of the disease.

Not but that Morton was filled with the theories of his times. "In a Scrofulous Consumption," he says, "the Blood, by

reason of its Preternatural Acrimony being, as it were, Coagulated and so unable perfectly to unite the new Chyle to itself and the solid parts, is wont to throw it in greater quantity than is convenient upon the Glandulous Parts where the small vessels do not run in a Right Line as in the muscles but spirally; from whence it comes to pass that the Blood is wont to make a longer stay and to make a more plentiful separation of the Nutritious Juices in them; whereby it comes to pass that these parts are apt to swell more and to grow larger and harder than others." This, as a theory as to why the glands were larger and harder than other tissues, might have appealed to Galen but it means nothing to us! Nevertheless, although the *theory* itself is wrong, it is expressed to explain an observation of great weight; the enlargement of the glands; and why should we quarrel with Morton for *thinking* incorrectly though observing so accurately?

That is why we applaud Morton so sincerely. Although his theories are those of a past phase, his *observations* are sound and true. They stand firmly amidst the welter of *views* that constantly trouble the ocean of opinion like rocks that remain firm in the ebb and flow of the tide.

BENJAMIN MARTEN

DR. CHARLES SINGER, in 1911, published in *Janus* an account of Benjamin Marten. "A Neglected Predecessor of Louis Pasteur," he called him. I do not know how Singer found out about Marten but he gives, at the end of his paper, the few places where the first and second editions of Marten's book are to be found; there are only four such in known existence. I read of Singer's discovery of Marten in a paper by Bulloch and at once got the second edition from the Royal Society of Medicine and, finding it of the greatest interest, proceeded to extract it; the results are published here. Afterwards I

obtained *Janus* and read Singer's paper, or so much of it as concerned Marten, finding out, as I had suspected, that very little was known about him. The "extracts," which form the principal part of Singer's paper, I deliberately avoided for I had already made extracts of my own and I was afraid that those culled by such an excellent writer as Singer, might prove to be very much the same as mine, much more systematically put, and that I might be accused of plagiarism if they were very like my own. It will be interesting to compare them with the latter when the article is finally published. Marten's book was published in 1722; the first edition must have appeared in 1719 but I have only the second to go by. It seems to me to be very well worth publishing, being, as Singer says, "neither more nor less than the Germ Theory of disease as we know it today." The full title of it is: *A New Theory of Consumptions, more especially of a Phthisis or Consumption of the Lungs*. It was printed for R. Knaplock at the Bishop's Head in St. Paul's Church Yard, and at several other places, in 1722. "These pages," says Marten in his preface, "have been out of Print for these Twelve Months past and, though much called for, were delayed . . ." etc. He adds, "There scarcely was ever any Book wrote and published, how Good and Correct in its Kind soever, but what met with Opposition, Censure or Ridicule from people of Ill Nature and of a cavilling Disposition, and that more especially if any Thing new or Uncommon was advanced in it." He quotes Harvey and Sir Isaac Newton as examples and goes on: "That the following Sheets therefore should escape the splenetic Reflections of Insidious Persons is as far from my Expectation as their Treatment of them and me will be from sharing any Part of my Concern."

I am indebted to Singer for two references, the originals of which I have looked up and shall here quote as showing that Marten was only too accurate in his expectations; his book savoured of rank heresy to the more correct part of the profession. The translator of Christopher Bennet's work was evidently of this type. He wrote in 1720, just when Marten's book was published, a preface to Bennet's work of which a part is here reproduced: "There needs no more to be said as to the

Perusal of such a Work as this than that it requires Judgement and Attention and is not to be run over like an ostentatious Farrago of Quotations which is collected more to answer the mercenary Ends of Empiricism than the worthy and just Intentions of true Knowledge; and of which the World has lately had an Instance of on this very subject." Another writer, Dr. N. Robertson, in his *New Method of Treating Consumption*, published in 1727, "Wherein all the Decays incident to human Bodies are mechanically accounted for," expresses his disagreement with Marten as follows: "Many Authors assert acid sharp corroding Humours; some a putrid sharp Blood; others a sharp saline volatile Lymph; and not a few have supported the Opinions of Worms and Animalculae's fretting or gnawing the Vessels of the Stomach, Lungs, Liver, etc., to be the immediate Cause of Consumptions," all of which he puts aside for his mechanical theory. No names are mentioned but Dr. Robertson points clearly to Marten as one of his examples. The very words, "Worms and Animalculae's fretting or gnawing," will be recognized by the reader as Marten's own!

That Marten had his critics and his enemies and that both raised a shout of opposition on the appearance of his book, may be judged from the two examples given. Alas! they succeeded for the space of a hundred and fifty years or more, in rendering his theories void and making his work to be forgotten. Nor should we blame them for this or forget that they were being true to the ideas of "dyscrasia" and "disposition" then accepted to explain the prevalence of the disease. It is one thing to put forth a *theory* as to germs or animalculae as the cause of disease and quite another to establish beyond dispute the *fact* that there is a germ capable of causing the trouble and that it may be seen, recognized, cultivated, inoculated into other animals, reproducing the disease in these for generation after generation. After all, Marten's theory was only a dream; a flight of fancy; an idea capable of explaining all if it proved to be true. But what a splendid effort! What a magnificent dream! And what a true one in the event! Marten was wiser than his critics. He anticipated, in thought, the *deeds* of Pasteur, Villemin and Robert Koch. "As to the Theory advanced in

the Second Chapter, I am sensible much more might have been urged in favour of it than the reader will find : However, I think I have said enough to evince the Reasonableness and Probability of my conjecture concerning the Prime and Essential Cause of Consumptions as well as of many other Diseases ; and to afford sufficient Hints to some abler hand whose abilities are more equal to the Task to carry the Theory much farther than I have done and, it may be, bring it to absolute Demonstration in an extensive Degree.” Avicenna, Fracastorius, and many others had thought, like Marten, of germs as the cause of tuberculosis or of disease in general, though none, it seems to me, had thought of them so *scientifically*, if one may be allowed to use such a word for a process of thought only, as Marten. And this laying down of the burden as beyond demonstration then but, perhaps, to be demonstrated later, in the fullness of a greater knowledge, was in itself a fine gesture. How tragic that the ingrained prejudices of the medical profession condemned such a man and such a work to oblivion so deep and so profound that it went right out of the consciousness of men and did not even act, as Marten had intended it to do, as a beacon to those that were to follow. He goes on: “ Could this be once accomplished, and I think I have not left it infinitely short of it, how soon and how much would the general Practice of Physic in Acute Distempers, Fevers especially, as well as in some Chronic ones, be alter’d from what it is at present ! ” His opinions were, we must admit, conceived in a veritable rebellion against the discursive and self-satisfied theories of his time ! “ I do verily believe ” he says, “ it may be easily proved that Animalcula are much more probably the true and direct Cause of the Plague we at this time so much dread than whatever else has or can be conjectured ; and that they are not the Effect only of a Pestilential Putrifaction, as supposed by a very learned and worthy Physician, who has lately published a piece on that Subject, but the real and absolute Cause of it, as I flatter myself almost all who read the Second Chapter with due attention will find reason to assent to.” It would be interesting to discover who the “ learned and worthy Physician ” was. I have studied the works of Sydenham on the aetiology of the plague but have

failed to find any such theory. No doubt many physicians wrote on the plague at that time whose works are no longer known to us.

This passage will serve to show, at least, that Marten applied his conceptions to a great many other diseases than consumption; here to the plague and, elsewhere to other acute and chronic diseases. But, as the title of the book shows as well as its contents, he spoke principally of consumption and its causation by minute living entities which he called "animalcula" but which I think, might, whether little animals or little vegetables, be spoken of as "germs." This wonderful theory we shall return to but, for the present, let us examine that part of the book which is laid out under Chapter I. Here we find a more natural, easy, accurate description than any heretofore given; a better approach to the modern idea of phthisis than even that given by Morton himself.

CHAPTER I

Chapter I, written without any particular mention of the theory which was to follow in Chapter II, ought itself to have made the book sell like wildfire if there had been, as yet, any great medical public for a true account of the disease. Perhaps, however, it was this that made a second edition necessary rather than any general agreement with the theoretical part which was to follow.

He begins: "For besides that a Consumption is in many forms an Original Disease or one not immediately proceeding from or occasioned by another, every other Chronic Distemper is many times resolved into it before Death compleats its Conquest." Here he roughly follows the teaching of Morton as to the varieties of consumption and, like him, speaks of the pulmonary type. "And indeed Custom has now so much prevailed with Physicians that whenever they speak of a Consumption it is generally and more especially taken for a Phthisis or that Consumption of the Body which has its rise from an Ulceration of the Lungs." Then, in a definition, he makes it clear, I think, that there was in his mind a difference between consumption of the lungs and other kinds of wasting; those due to diabetes,

to osteo-myelitis, to malignant disease, etc; "A Phthisis or Consumption of the Lungs may be very justly defined to be a wearing away or consuming of all the muscular or fleshy Parts of the Body, accompanied with a Cough, purulent Spitting, hectic Fever, shortness of Breath, Night Sweats, etc." This description, clear enough, it seems to me without any introduction of autopsy findings or anything complicated, is made even more definite by the following: "Under all which they are supported only by Intervals of Hope that they shall at last overcome their Distemper and meet with a perfect Cure; which Hope, being the greatest Comfort they have, they very justly endeavour to keep up in their Minds as much as possible and are fatigued with nothing more than to be discouraged by their Friends and Acquaintances from that their only Enjoyment they seem to have."

Marten then proceeds to quote various authorities on the kinds of person especially supposed to be very prone to phthisis and I shall place in italics those portions which seem to me to be worth notice. "The divine Hippocrates and from him several others tell us that Persons with a fine Contexture, tender, and who have a small shrill Voice, thin clear Skin, a long Neck, narrow Breast, depressed or strait Chest and whose shoulder blades stick out are of all others most subject to Consumption; and this is in some measure confirmed by Experience *but must not be taken as a general Rule because we often find robust and strong men fall into this Distemper and such weakly tender Persons as above described many times exempted from it.*"

"Consumptive People are likewise generally observed to be very full of Spirit, hasty and of a sharp and ready Wit . . . *But that only ingenious Men are seized with this Distemper cannot be said though it is certain that we do not often meet with dull heavy Persons or such as are slow of Speech affected with this Disease.*"

"People between the age of eighteen and thirty-five are much more subject to a Consumption than those who are either younger or older *yet this must not be taken as a Rule neither.*"

These iconoclastic reservations are quite characteristic of the man. They show that independence of authority which was, perhaps, his highest attribute. But he modifies his conclusions

just where I should have had him do. "It is observed also that Persons born of Consumptive Parents and such as are prone to spit up a black Flegm in a morning . . . or that spit Blood, though it be only accidental, through loud Hollowing, Singing, Hard Coughing, Running or any violent straining . . . are much more liable to a Consumption than others, and this experience confirms." But he was liable to make mistakes as well as to correct them. "This Distemper is, in the opinion of several, propagated from one Person to another by Contagion and, if we may give credit to a certain Author, is very infectious indeed; for, speaking of a Consumption, he says 'That considering its Malignity and Catching Nature, it may be conumerated with the worst of Epidemics since, next to the Plague, Pox and Leprosy, it yields to none in Point of Contagion. . . . Nothing (adds he) taints sound Lungs sooner than inspiring (or drawing in) the Breath of putrid, ulcerated or consumptive Lungs.' . . . But," says Marten, "if a Consumption of the Lungs was so exceedingly Contagious as this Author would have us believe, every one in a family wherein a Consumptive Person lives, as also all his Acquaintance . . . would be infected with this Disease, which, however, experience contradicts." But this is exactly what does happen! And it is what is bound to happen either on the "certain Author's" or on Marten's own theory of the disease. Everyone in close contact with an open case is *infected*; but this is a very different thing from *contracting the disease*. Neither of these authorities could be expected to know of the results of the tuberculin test, however, and we quite know what Marten meant by his criticism. Nevertheless we should like to know who the other author was as well. He would seem to have been before Marten in his theory of consumption.

Marten quotes Entmuller as follows: "A Phthisis is so very contagious that the Breath of a Phthisical Person will corrupt the lungs of another and convey its Virulency through the Breasts of a sucking Woman." He also quotes Morton who says: "For this Distemper, as I have observed by frequent Experience, like a contagious Fever, does infect those that lye with the Sick Person with a certain Taint." Marten, in

fact, makes it quite clear that he knew of many that had spoken of phthisis or consumption as a disease sometimes or always conveyed by infection from one person to another; these however being rare enough or inconclusive enough still to allow him to speak of his own theory of the disease as a "New Theory of Consumptions."

We pass on to what Marten considered to be the early stages of the disease, specially likely to be neglected as being so like an ordinary cold: "A Consumption of the Lungs is often introduced in the following manner: The Patient from being, to all outward appearance, in perfect Health and Strength is, upon taking Cold . . . first seized with a Defluxion of thin Rheum from the Nostrils, a soreness of the Palate, Throat, Breast and Lungs, which is soon attended with a Hoarseness and a troublesome Coughing up of Matter, at first thin and white, then bluish, equable and globular, but, in a short time, yellow and green resembling Pus or Matter commonly found in external Ulcers. Yet none of these Symptoms discover the Lungs to be so affected as the Wind-pipe and its Glandulous Membrane . . ."

"For the most part those who are seized with these fore-running Symptoms or first steps towards it in the Winter Season are not affected with a Consumption so acute or speedily hastening to its End as those who are first attacked in the Summer, who are generally such as are in the Vigour of their Youth and whose Blood is commonly very hot and acrid. In these the Hoarseness is very great on a sudden and to the Symptoms before-mentioned is frequently added that of bringing up by Coughing almost from the very beginning a purulent Matter streaked with Blood." It is to be noted that Sydenham, too, noticed this concentration of very bad cases in the early spring and summer. I met the same phenomenon in Wales where the early adult type of tuberculosis was still rife in 1922 and later. There was always an increase of bad cases and deaths in April, May and June. It seemed to me that the young adults chiefly affected had got their infection from the more chronic types confined to the houses during the winter and had developed their rapid symptoms during the spring; but the phenomenon is difficult to understand. London was, in the days of Sydenham and, later,

of Marten very like the Wales of 1922 and 1923, a hot-bed of the young adult type.

"Amongst the imminent Symptoms . . . must also be reckoned Haemoptoë or spitting of Blood which sometimes happens on a Sudden and in large quantity without any previous Cough; . . . and when severe is quickly attended with a Train of Symptoms threatening speedy Death; and if the Patients escape with Life they are nevertheless in imminent Danger of falling immediately into a very acute and deplorable Consumption." Who has not met cases going from a fairly healthy state into a sudden decline after an extensive haemoptysis that has swept the bacteria from their relatively harmless situation in a chronic cavity into the vast unprotected spaces of the lungs, hitherto uninfected except for the small germ-laden vomica concerned? "I call all these Symptoms the first Steps only towards a Consumption because if due and timely care be taken they may easily be cured," says Marten, but "if special Regard be not had to these dangerous Symptoms . . . they soon hurry them into a consumptive state which, of all others in Life, is surely the most deplorable because, besides the Fatigues of the Body the unfortunate Labourants undergo, they see themselves as it were dying every moment they live." I imagine that the genuine tuberculosis cases were the ones that were less easily cured and that a great many of the others were not tuberculous at all though greatly resembling it. And there is the probability that many real cases took a temporary hold on themselves when put at rest and were thought by Marten to be "cured" though they may have broken down later! I do not think that many would agree with Marten that their symptoms, as described, were "the first steps towards a Consumption."

"But there is a Phthisis . . . of a slower Nature than the above mentioned," says Marten, "and which takes up a longer time in passing through its several Degrees . . . These first steps towards a slow or lingering Consumption generally happen to such as are hereditarily disposed to it . . . Now a Phthisis . . . that is occasioned by other Distempers is called a Symptomatical Consumption . . . Yet we often find a lingering consumption takes place in Persons that were never subject to

or afflicted with those chronic Distempers and then it is called an Original Consumption." Here he applies Morton's classification but is obviously somewhat puzzled by it! Why should persons afflicted by a lingering consumption be, as Morton says, of the category of "Original Consumptions"? Were they perhaps, Symptomatical after all? "And here I cannot forbear again observing that many of these Symptoms . . . such as Cough, Hoarseness, Catarrh, etc., being the Effects of Common Colds, . . . are on that account too often neglected by the Patients till a Train of innumerable Ills happen."

Finally, as an end to our summary of Chapter I, we quote what must have appeared almost an amusing epigram to Marten himself: "A violent Cough (as the learned Baglivi says) produces a Rupture in Children, Miscarriage in Women and a Consumption in Men, for which reason it ought never to be slighted!"

"Amongst these enumerated Symptoms of an incipient Phthisis, or Consumption of the Lungs, in its first Degree, there are some few that are accounted *Pathognomonick*, or inseparable Signs of that Disease, or such as are peculiar to it, and always attend it, and those are the dry hectic Cough, Hectic Fever, and Wasting of the Muscular or fleshy Parts of the Body." *Remark that these were accounted phenomena of the disease in its first degree!* This passage shows that clinicians then regarded these signs of very far advanced disease as *early signs*. This remained true of the average medical man until the X-ray carried conviction to even the least expert. Marten talks of vomicae and their results. He is alive to the danger of empyema as the following extract shows: "But when this large Apostem happens remote from the Branches of the Wind-pipe, it then, upon breaking, fills the whole Substance of the Lungs with putrid Matter, swells them and at length corrodes and breaks through their containing Membrane, and is emptied into the Cavity of the Breast and forms the Disease call'd an *Empyema*, or a Collection of Matter in the Cavity of the Chest, which is not to be cured without the greatest Difficulty and utmost Skill." The classification of the stages of phthisis is now given. "And as the Tubercles, Knots or Swellings that I have mentioned beginning to infest the Lungs, in the first Degree of a Consumption,

either acute or chronic, are inclined more or less to be inflamed, a new Fever properly called Inflammatory and Continual, is sooner or later joined to the Hectic Fever that before afflicted the Patients . . . The Consumption, when the Tubercles incline to an Inflammation, and the Inflammatory Fever begins to invade, may then be said to have arrived at the Second Degree."

The pulse, "when felt with several fingers together, rises up more in one place than another, so as to feel, as Morton says, somewhat like a Saw. . . . When the fever has lasted about seven days, it stops for a time, the inflamed Tubercle in the Lungs that produced it being in that space ripened and broke; from which we may date the Third Degree of a Consumption." As to the last stage; "They feel a kind of Hollowness within the Breast as if their chest was quite empty; their Backbone, Ribs and Bones of the Brest stick out whilst the Belly sinks in and seems tucked up almost to the Back as if they had no Bowels; their Cheek-bones stick out; the Face is pale, livid or swarthy; their Nostrils are sharp, the Skin of the Forehead and the Palms of the Hands becomes hard and dry; and they appear very little better than Skeletons covered only by wrinkled Skin . . . which may be called the Fourth or Last Degree of it."

CHAPTER II

This Chapter contains and expresses all the essential matter of the "New Theory of Consumption." It begins with a summary of older ideas of which an example may be given. Phthisis was originally attributed by the ancients, Hippocrates, Galen, etc., to a "sharp corroding Humour or Salt Flegm that, as a Catarrh, falls from the Brain upon the Lungs which, being excessively soft, tender and very apt to corrupt, it exulcerates and breeds a putrid Ulcer. . . . But Helmont very much ridicules this opinion of a Catarrh falling from the Brain as being wholly groundless and the Moderns, since the Discovery of the Circulation of the Blood by the Immortal W. Harvey, being much more accurate in Anatomy than the Ancients were, sufficiently explode that Notion as entirely fabulous."

He then mentions the theories of Paracelsus and his followers,

of Helmont, Sylvius, Willis, Dolaeus, Entmuller and Morton, of the last of which, however, he says : " Morton also speaks of some Peculiar Malignant or Ill-natured Quality in the Fluids," and he includes " Infection " as one of the pre-disposing causes. " But," says Marten, " I believe we must seek some other Original and peculiar Cause of this Distemper before we have a satisfactory Idea of it." He then points out that the six non-naturals, though important, cannot be the real factors of the start of a phthisis. " Yet I cannot help thinking that these are only secondary Causes that accidentally aid and promote some other Peculiar, Latent or Essential Cause which I suppose to be joined with them." Then, suddenly, he utters his real opinion : " The Original and Essential Cause, then, *which some content themselves to call a vicious Disposition* of the Juices, others a Salt Acrimony, others a strange Ferment, others a Malignant Humour (all which seem to me dark and unintelligible) may possibly be some certain Species of *Animalcula* or wonderfully minute living creatures that, by their peculiar Shape or disagreeable Parts are inimicable to our Nature; but, however, capable of subsisting in our Juices and Vessels."

Here we have the Theory at last! Phthisis—and other diseases as well—caused by " wonderfully minute living creatures." Whether animalcula or vegetable organisms he could not know. He called them " animalcula " provisionally but they might evidently be anything else! There is nothing to exclude a *vegetable* organism—in that dim penumbra of attribution where we place organisms in one category or the other—in the expression " wonderfully minute living creatures."

In the copy of Marten's book which we have, at present, at our disposal from the Royal Society of Medicine, there exists on page 41, a note in a very faint old writing as follows : " If so, it cannot come to anyone hereditarily as mentioned on Page 9 unless you allow p. 45, etc." On page 9, the book says: " These first steps towards a slow or lingering Consumption generally happen to such as are hereditarily disposed to it." On page 45 it adds: " Nay, it will not be difficult to suppose that, as the Fluids of our Parents might abound with many

Species of Animalcula, so the Ova or Eggs of them may have been communicated to us or conveyed into our Fluids with the Nourishment we receiv'd through the Umbilical Veins." This is a first-rate criticism though it was allowed for by Marten, as the second extract shows. We agree, nowadays, that "minute living creatures" are the cause, and it appears, in very exceptional cases, that they may gain access to the foetus through the placenta; the matter remains doubtful but was advocated by Calmette and one of his co-workers, Valtis, a few years ago.

Marten proceeds: "This Opinion of Animalcula or exceedingly minute Animals that are noxious to our Nature being the Cause of a Consumption of the Lungs will doubtless seem strange to abundance of Persons and, more especially, to those who have no Idea of any living Creatures besides what are conspicuous to the bare Eye; but the Curious, who have not only employed themselves in the Philosophical Studies of the vast Machinery of the Universe but have also turned their Thoughts upon the admirable Works of Omnipotence in the *Minimae Naturae* and have consequently considered the new World of Wonders that Microscopical Observations have opened to our View, will easily conceive the possibility of very minute Animals being not only the Original and Essential Cause of this but of many Distempers hitherto inexplicable; and that they are, perhaps, the very *Malignity* so much complained of in many Distempers but so little understood. . . . So we may reasonably conclude that there are Myriads of others, infinitely smaller, and wholly imperceptible to our Eyes though assisted by the best Glasses that can be made. . . . From which it may be easily conceived that as these little Animals at their full growth are too small to be perceived without the help of Glasses, so their Ova or Eggs must be exceedingly smaller and consequently so light as to be capable of being carried to and fro in the Air and so may be sucked in with our Breath . . . or those very minute Creatures may perhaps deposit their Ova or Eggs in the Fruits of the Earth or in the very Food we eat . . . by which means they may get into our Blood and Juices. . . . In which Case if they meet with a proper Nidus or Nest in any of our innumerable Vessels, they are then brought

to Life and, by this Growth, disagreeable Parts and Spontaneous Motion, may cause many Diseases besides that we treat of." Marten then discusses the production of mites in cheese which he thinks—wrongly of course—might be first in the animal producing milk, then in the milk and, surviving heating, later in the cheese. He goes on, however; "but I conjecture that it is very possible that several Distempers which we do not receive hereditarily from our Parents, may be caused by Animalcula inimical to our Nature, the Ova or Eggs of which may be conveyed into our Blood and Juices either with our Food, our Breath, or even by insinuating into the external Pores of the Body. . . . And we may reasonably imagine that each Species of such Animalcula . . . keep together in vast numbers . . . as shoales of small Fish and Swarms of Flying Insects . . . so we may easily conceive, if the Theory be admitted, how it happens that sometimes a pernicious Disease becomes Epidemic." Here we may reflect upon some, at least, of the things that are still hidden from us about epidemic diseases. How, for instance, was the Influenza that overcame us in 1917, the whole of an army laid out with it in three or four days, propagated from one to the other? Were the germs disposed "like Shoales of small Fish?" We cannot tell. What we feel, what we almost know, is that "contact" had nothing to do with it. "How Distempers happen to rage in one Year or Season of the Year, or in one County more than another . . . and how they are spread by Degrees and from one Country to another, may by this Theory," says Marten, "be more easily explained than by any other."

He then very wisely turns to diseases with one of which he was quite familiar and which exemplified his theory of an animal spread by "contact" very well. "We find some Diseases are apparently communicated from one person to another by Contact or Touch only, of which the Itch and the Venereal Distemper are eminent Instances; that the Essential Cause of the former Disease is a vast number of minute Animals that by their spontaneous Motion and injurious Parts make Furrows under the Epidermis or Scarf Skin may be demonstrated by viewing with a good Microscope a small Portion of the Humour

contained in the little Bladders that arise in that Distemper between the Fingers, etc., in which may be perceived abundance of ill-shaped Creatures that, by nestling under the Cuticle or Scarf Skin, stimulate the nervous Fibres and cause the troublesome Sensation of Itching, etc. Some of these minute Insects, contained in a very small Part of that Humour, fixing upon a sound Person's Skin, either by shaking Hands with an Itchy Person or wiping with the same Towel, or drawing on one of his Gloves, will soon insinuate into the Pores and there increase and multiply and quickly act the same Part in that Person as in the other from whence the Disease was received. . . . For, in the Itch, if Sulphur, which will often cure it or Mercurials which always will, be not closely followed according to Art, so as not only to check but also to utterly destroy all the minute Animals that, as we plainly see, cause this Disease, it in a short Time breaks out again and fatigues the Patient as much as ever." He speaks of the venereal infection, with which, of course, he was, as we were until the work of Schaudinn led us to the truth, quite in the dark, and postulates the changes of type from one appearance to another of minute organisms causing disease. "And that many species of small Animals do indeed change their Shape or have different external Parts unfolded at certain Periods of their short Life than what appears" is confirmed by the account of Dr. Hooke (1635-1703), author of *Micrographia* (1665). Here Marten quotes Hooke's account of the metamorphosis of the gnat of which the larvae are kept in water for three weeks.

Returning again to the suggested causation of pulmonary consumption by "minute living creatures," he rises to a height of prophetic accuracy which is almost incredible. "It seems much more probable that the minute Animals or their Seed . . . are for the most part either conveyed from Parents to their Offspring hereditarily or communicated immediately from Distempered Persons to sound ones who are very conversant with them. . . . It may, therefore, be very likely that by habitual lying in the same Bed with a consumptive Patient, constantly eating and drinking with him or by very frequently conversing so nearly as to draw in part of the Breath he emits

from the Lungs, a Consumption may be caught by a sound Person. . . . I imagine that slightly conversing with consumptive Patients is seldom or never sufficient to catch the Disease, there being but few if any of those minute Creatures . . . communicated in slender conversation and which, if they are, may not perhaps be produced into Life or be nourished or increased in the new station they happen to be cast . . . But when the Ova or Seed of the pernicious Animalcula are constantly getting into the Blood and Juices . . . some of them may be produced into Life." Here is explained the communication of infection "from distempered persons to sound ones," the danger of cohabitation, and yet the very slight or altogether absent risk in just talking or slightly associating with tuberculous persons. All this prevision is quite extraordinary. And yet, in his humility, Marten adds: "I have treated of what I have advanced only as conjectural and barely possible, well knowing that Disquisitions of this Nature lie too deep for us easily to demonstrate."

CHAPTERS III AND IV

In these chapters, which are chiefly about treatment, we can safely ignore the various prescriptions given but we must, at least, quote a few of the deductions. Here is one: "From what has been said, I think it evidently appears that Evacuations are necessary for the absolute Cure of Consumptions but the great Difficulty is to find out such a Method of Evacuation as can be bourne by the Patients even in the advanced Phthisis, regard being had to the weak state we commonly find in them. . . ."

"Bleeding, in this Distemper, is very often found to diminish the Patient's strength exceedingly. . . . We must therefore direct Bleeding with the utmost Caution and Circumspection. . . . Another thing that we must have a principle Regard to is not to load our Patients with too many Medicines. . . . During this Course of Remedies proper Exercise must also be used and that more or less according to the Strength of the Patient, state of the Disease, etc. . . . And here it may not be amiss to observe that Consumptive Persons for the most part are able to use a great deal more Exercise than themselves many

times think probable. . . . When People labouring under this Distemper are continuously sitting still, musing on and lamenting on their own sickly condition and constantly confine themselves to their Houses and Chambers . . . they take a ready means to promote the Distemper instead of their own Health."

And then Marten begins to speak of *riding* as a most valuable addition to all other exercises and gives a very sensible account of how it may be regulated so as not to be either too violent to too slight. The reader will remember what was said on this subject when it was discussed under Sydenham's remarks. Marten's advice is, if possible, more to the point. "And of all others that of riding on Horseback in the manner of Travellers and not furiously is certainly the most excellent, which advantages cannot be said to occur from Walking, though of great service, too, where the other cannot be complied with. . . . When they first begin the Exercise they are commonly fond enough of it but, if they ride without Company, then, after having once visited the adjacent Villages, they become indifferent. . . . On the other hand, if they ride out in a Company, they too frequently either ride too long together or too violently by Fits and Starts. . . . Riding out a few miles only once or twice a week, which some content themselves with, cannot be expected to answer the end any more than riding too hard, that is to say too violently or too many miles a day so as to be exceedingly fatigued or quite tired, and then to omit the exercise for a week or fortnight afterwards cannot be supposed to be serviceable. . . . And the best Method that I know of, which even People who have continued Business in Town may comply with, is to have their Families in the Country at about eight or ten miles from London and constantly go thither in the Evening and return to Town early every Morning. . . . Their Minds, also, will be much better entertained while they are on Horseback this way, which is a kind of Business, than if they were riding out to take the Air. . . . By it and a few well-adapted Remedies, as the case may require . . . I am confident many Persons may be restored to Health and Strength and, as I may say, to a new Life."

There are many other bits of advice which the author gives

unrestrictedly to his readers, but I shall not quote them here. I shall, however, add two aphorisms :

“ But, in general, as to Diet and Rest, the most regular Method is certainly for People to rise from Bed about six or seven in the Morning, to breakfast about seven or nine, dine between twelve and one, sup about seven or eight in the evening and go to Bed about ten.”

“ And now, although I am not of Opinion that Consumptives are incurable at the time or near so soon as many others think them, yet certain it is that the sooner such Patients are put into a regular course for Cure the sooner and more certainly will the Distemper be effectually overcome.”

And so we bid a reluctant farewell to Benjamin Marten, one of the most vivid and intelligent writers upon consumption that we have so far met with and by far the most imaginative and successful of all those who tried, before the coming of Pasteur and the arrival of Koch, to sketch out with a firm and skillful touch a theory of the germ-cause of this formidable disorder. We shall speak of William Budd in due course and give him a mead of praise ; but we cannot but regard the work—shall we call it—of Marten as much the more remarkable of the two. His prognostications as to the infectivity of man, the means of spread and the importance of “ contact ” strike one as a dream so vivid and true to subsequent discovery as to be almost an inspired statement—forgotten as it was and only to see the light of day after Villemin and Koch had brought the *reality* of the cause of phthisis to its proper place in the altitudes of medicine.

WILLIAM STARK (1741-1770)

WILLIAM BULLOCH, in his Horace Dobell Lecture on November 10, 1910, a lecture which dealt with "The Problem of Pulmonary Tuberculosis," referred to the work of "William Stark, a young physician of St. George's Hospital, whose untimely death at the age of twenty-nine, brought about by experiments on his own body, robbed English pathology of one of its earliest and most accurate observers." There is no doubt that Bulloch was right in his assessment of Stark as one of the earliest and most accurate of British pathologists nor is there any doubt that he was a physician of St. George's Hospital, yet it is a curious fact that St. George's Hospital has now no trace of this brilliant experimentalist in its notes of former distinguished St. George's men. The reason would appear to be that Stark studied first in Glasgow and then in Edinburgh and that he only came to London and to St. George's later on. He was never strictly a student there if we are to accept the rather limited idea of a student as a neophyte in the study of a profession. He had been a neophyte in Glasgow and in Edinburgh but it seems that he was only a very keen and enthusiastic seeker after truth when he came to St. George's in the year 1765 and now "devoted himself to the study of physic." (Dr. Carmichael Smith's preface to the works of Stark, 1788.)

There is no doubt that he was very well known to his colleagues at St. George's at that time and was remembered there for long after his death. Matthew Baillie seems to have completely confirmed his work in 1793 and the name of the latter is coupled with his by Bulloch as one of the founders of the studies underlying "the modern era of tuberculosis."

It is sad to see so much learning and so much endeavour now quite forgotten amongst the very scenes where it was acquired; but it is comforting to remember that the knowledge

itself was well used by a number of brilliant St. George's men in after years. No hospital has contributed more generously to our understanding of tuberculosis than St. George's itself.

To Dr. Carmichael Smith, another St. George's man, we owe the publication of Stark's works in 1788. Without his valuable assistance in this matter they might have gone unpublished and we should be so much the poorer in this respect. In his preface, he gives the following brief biographical note: "Stark's father, as I have been told, was a native of Ireland, his mother of Scotland; he himself was born in Birmingham . . . But, wherever his cradle was rocked he was educated at Glasgow and there, under Drs. Adam Smith, Black, Reid, etc., he first learnt the rudiments of philosophy . . . From Glasgow he repaired to Edinburgh where he was soon distinguished and honoured with the friendship of Dr. Cullen . . . Having finished his studies at Edinburgh he came to London in the year 1765 and now devoted himself to the study of Physic and, looking upon anatomy as one of the pillars of the Art, he endeavoured to complete, with Dr. John Hunter, what he had begun with Dr. Munro, and acquired under this eminent Professor that perfect anatomical knowledge which appears in all his dissections. He likewise entered himself a pupil at St. George's Hospital and, disgusted as he often told me, with the inaccuracy and want of candour of the generality of practical writers, he determined to obtain an acquaintance with diseases at the latter School and under an abler master; and to have, from his own experience, a standard by which he might judge the experience of others. . . . In 1767 he graduated at Leyden and published an inaugural Dissertation on Dysentery. On his return to London he recommenced his studies at the Hospital and in June 1769 began his Experiments on Diet, to which undertaking he was greatly encouraged by Sir John Pringle and Dr. Franklin, whose friendship he then enjoyed, and from whom he received many hints, both as to the plan and afterwards in the execution of his design. These experiments, or rather the imprudent zeal with which he prosecuted them, proved in the end fatal to himself; at least such was the general

opinion of his friends at the time but, to my mind, and I speak from intimate knowledge of his character, other causes, particularly chagrin and disappointment, had no small share in bringing about the event. . . . Dr. Stark was much more conversant with books than with men. . . . He was ill prepared for the cold prudence, the time-serving meanness or the base duplicity which he met with in others . . . nor had he yet made the observation of Figaro, equally applicable to all ages and to all countries,

' que le savoir faire vaut mieux que le savoir. ' "

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What did Stark do for our knowledge of tuberculosis? To me it seems that he did much. At a time when the great majority of doctors was content to study the gradual march of the disease from its diagnosable first stages to its final breakdown into a fatal illness he made the essential first studies which were destined to lead pathologists and, finally, clinicians to perceive that the disease might, in some cases, be amenable to cure. Others had, it is true, seen tubercles in the lungs and had noted the presence of "apostemes" and "ulcers" as part of the disease complex, but Stark was the first to examine minutely the growth and development of these little tumours and to show how they might gradually lead to advanced disease and death. Let him speak for himself. "In the cellular substance of the lungs are found roundish firm bodies, of different sizes, from the smallest granule to about half an inch in diameter, the latter often in clusters. The tubercles of a small size are always so; even those of a larger are frequently so; they are of a whitish colour and of a consistence approaching to the hardness of cartilage; when cut through, the surface appears smooth, shining and uniform. No vesicles, cells or vessels are to be seen in them, even when examined with a microscope, after inspecting the pulmonary artery and vein. On the cut surface of some tubercles were observed small holes as if made by the pricking of a pin; in others were found one or more cavities containing a thick white fluid like pus; at the bottom also, of each of these cavities, when emptied, several small holes

were frequently to be seen from which, on pressing the tubercle, matter issued; but neither these holes nor any others above-mentioned (so far at least as could be determined) communicated with any vessels . . . The cavities, in different tubercles, are of different sizes, from the smallest perceptible to half an inch in diameter and, when cut through and emptied, have the appearance of small white cups, nothing remaining of the substance of the tubercle except a thin covering or capsule. . . . The cavities of less than half an inch in diameter are always quite shut up; those which are a little larger have, as constantly, a round opening made by a branch of the trachea. At this period, there being a free passage for the matter contained in the tubercle into the trachea and a communication between the cavity of it and the open air, it is proper to change the name of tubercle to that of vomica."

"The smaller vomicae are commonly entire, the larger are frequently ruptured (which generally speaking, are of an oval shape and about four inches in length) are lined, either partially or entirely, with a smooth, tender slough or membrane; the same as the capsula of the smaller vomicae. The matter contained in them, when the capsula is entire, is whitish or yellowish; when ruptured reddish; in either case readily diffusible in water. It is proper, however, to remark that, even in the largest vomicae, when they are not completely ruptured, the matter is seldom red but yellowish, ash-coloured or greenish; often foetid. . . . Into all the vomicae, the smallest excepted, there are several openings of the bronchia; also openings forming communications between the different vomicae. The bronchial openings are commonly round and smooth, the others generally irregular and ragged. The larger vomicae, which have numerous bronchial openings, are found to contain scarcely more matter than is sufficient to besmear their surface; and what shows clearly that the matter is discharged by these openings of the aspera arteria is that if a deep incision be made into any diseased part of the lungs, and that part gently compressed, the matter will be seen to issue from the cut extremities of the bronchia; or if any considerable branch of the aspera arteria be laid open and the lungs pressed in the same manner,

the matter will be seen coming into it from the smaller ramifications. . . . Where ever there is a vomica there is always a broad and firm adhesion of that part of the lungs to the parietes or pleura so as to preclude all communication between the cavity of the vomica and that of the chest; even tubercles are seldom without adhesion."

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"The pulmonary artery and veins, as they approach the larger vomicae, are suddenly contracted; a blood vessel which, at its beginning, measured nearly half an inch in circumference sometimes, though it had sent off no considerable branch, could be cut up no farther than an inch; and when, outwardly, they are of a larger size, yet internally they have a very small canal being almost filled up by a fibrous substance; and, frequently as they pass along the sides of the vomicae, they are found quite detached for about an inch of their course, from the neighbouring parts. That the blood vessels thus are obstructed and that they have little or no communication with the vomicae, is rendered still more evident by blowing into them or injecting them; by blowing into them they are not swiftly distended nor does air pass into the vomicae except very rarely and then only by some imperceptible holes and, after injecting the lungs by the pulmonary artery and vein, the parts less affected by disease, which before injection were the softest, become the hardest and *vice versa*, the most diseased parts, before injection the hardest are now the softest. . . . The wax was very rarely found to have entered the middling sized vomicae and never the smaller or larger ones."

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State of the air vesicles and cellular substances. Those parts of the lungs which are contingent to tubercles are red, sometimes soft, but more frequently firm and hard; and, while other parts of the lungs unaffected by disease are readily distended by blowing into the trachea, those parts which are contiguous to tubercles or vomicae remain depressed and impervious to air, either blown into the lungs in this manner or forced, by a blow pipe, into incisions made on the surface. So that the

function of the lungs, so far as respects the admission of air, seems, in those parts, entirely destroyed."

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Stark gives an excellent summary of the symptoms of the disease but I think I have quoted him sufficiently to show what a pathologist was able to do towards the unravelling of the tangled skein of tuberculosis in 1769.

JAMES CARSON (1772-1843)

DOCTOR JAMES CARSON, "Physician to the Workhouse Fever Hospital and the Asylum for Pauper Lunatics at Liverpool, and in Charge of the Military Hospital at that Place;" thus he is described on the title-page of what appears to be his earliest book, *An Enquiry into the Causes of the Motion of the Blood*, printed by F. B. Wright, Liverpool, in the year 1815. The description of him, in 1822, in his further work, *Essays, Physiological and Practical*, is simply "Physician in Liverpool" but there is no reason to suppose that he had abandoned any of his appointments at that time. There was also his private practice which must have made great claims on him. How, then, did he manage to carry out his very important physiological researches on the elasticity of the lungs, the motion of the blood and the contractility of the arteries in addition to all these appointments? How did he secure the amount of leisure to give medical evidence at a prolonged and complicated trial, that of Charles Angus, and to defend his position in the Press? ("A Vindication of the Opinions delivered in Evidence by Medical Witnesses for the Crown on a late Trial in Lancaster.") The answer, I think, must be that he was a Scot, fired with the consuming ambition sometimes met with in men of that fervid nationality to discover things hidden from others; why the lungs adhered to the moving cradle of the chest wall, for

instance; why the arteries should be empty of blood when opened after death; what would happen if the chest wall were to be incised, allowing access to the outside air? These were the problems for a Scot! He *made* time for them. James Carson was at first educated for the Ministry but he turned from the study of divinity to that of medicine and graduated at the University of Edinburgh in 1799. He came to Liverpool later where he lived and practised his profession. His physiological researches must have been appreciated by the leaders of medicine at that time for he was made a Fellow of the Royal Society in 1837. Lawrenson Brown, in his essay on artificial pneumothorax, (Lawrenson Brown, M.D. *The Story of Clinical Pulmonary Tuberculosis*. Baltimore, The Williams and Wilkins Coy., 1941), an excellent work giving much information, mentions that Carson was "known to many of his contemporaries and to some Scientists who followed him; to Canstall (1843), to Parola (1849), to Wunderlich (1856), and to Kohler (1867) among others." There seems to be no doubt that he was deservedly well known to many. The only thing that is unaccountable is that such a scientist and such a liberal contributor to human knowledge should have been so largely forgotten! Everyone understands the principles of artificial pneumothorax nowadays and every Tuberculosis Officer or other chest expert practises it. Everyone is familiar with the name of Forlanini, deservedly well known as the introducer of this little operation, so effective in pulmonary tuberculosis; but why is the real and authentic introducer of the method left so severely alone? Carson introduced to the medical profession the possibility of artificial pneumothorax, explained the principles underlying it, described physiological experiments on the adherence of the visceral to the parietal pleura under normal circumstances, spoke of the retractility of the lungs if the outside air were allowed to obliterate the negative pressure in the pleural cavity, the safety with which one lung might be allowed to collapse and the bearing of all the facts upon the healing of cavities in the lung. Surely his name should be not only remembered but honoured as the true discoverer of artificial pneumothorax!

Let us turn to his short but pregnant description of his work on the collapse of the lungs :

On the Elasticity of the Lungs.

“If a piece of the substance of the lungs be cut out and stretched it will recover its former dimensions when released from the extending power. . . . But though the existence of this property had been universally admitted, no physiologist had attempted as far as I know, to explain the means by which nature had contrived to render it subservient to the purposes of life.” He then proceeded to do so. By means of a containing bulb to which are sealed by means of a blow-pipe tubes, “one nearly three feet in length and bent up at one end,” the other a shorter tube joined to the bulb at the other and then fixed, in a manner impervious to air, to the wind-pipe of an animal recently killed, he was able to observe, after filling the bulb with water as well as the longer tube to an appropriate height above that in the bulb, the movement of the water in the long tube on opening the chest wall or the diaphragm of the animal under experiment. He found that the water column was pushed up for a distance of about six to eighteen inches according to the size of the animal as soon as the pleura was opened to the outside air. “But the lungs,” he concludes, “are powerfully elastic and, when distended to the dimensions which they are thus forced to occupy, their substance is stretched far beyond its natural condition. As soon, however, as the full weight of the atmosphere shall be allowed to ponderate upon the external surface of the lungs and an equal pressure shall be sustained by the external surface and the interior surface of the air vessels of which their substance is composed . . . these organs will shrink into the dimensions which are prescribed to them by the nature of their structure.” By these means he was able to show that “in calves, sheep and in large dogs, the resiliency of the lungs was found to be balanced by a column of water varying in height from one foot to a foot and a half and in rabbits and cats, by a column of water varying in height from six to eight inches.” After a paper on *The Vacuity of the Arteries after Death*, which, though of great interest, does not require quoting here,

he then proceeds to his third article, presented to the Literary and Philosophical Society of Liverpool in November, 1821; *On the Lesions of the Lungs*, a short paper and yet one in which the principles of artificial pneumothorax treatment of phthisis and other chronic ulcerative disorders of the lungs are beautifully handled and the means of cure, in appropriate cases, made clear.

"The experiments," says he, "with a detail of which this paper commences, were instituted with a view to supplying a surer and more ample foundation for the observation I am about to hazard respecting the peculiar character of lesions of the lungs and of the course which ought to be observed in our attempts to cure these lesions."

"The chief cause of the peculiar obstinacy observed in the healing of injuries of the lungs arises, in my opinion, from the state in which these organs are held in the living system. It has been proved that the substance of the lungs is powerfully elastic and that, in the living system, it is at all times on the stretch. When the lesion, from any cause, occurs in the lungs, the sides of the divided substances recede in opposite directions; and a power equal to the elastic spring of the fibres tends not only to prevent the approach of the divided parts but still further to increase the breach. . . . But in the case of an abscess of the lungs, though a vent may be obtained for the discharge of the matter, this matter will still remain in the abscess or will only be discharged when its place has been supplied by an equal bulk of other matter or of air. For the elasticity of the substance enclosing the abscess causes a retraction of that substance on all sides in a direction opposite to that of the centre of the abscess. The cavity thus formed becomes independent of the matter it may contain. The sides of the abscess are prevented from falling into a salutary contact, not by the matter which lodges between them but by the powerful elasticity and retraction of the surrounding substance. . . . It is evident that, if one of the lungs were reduced to a state of collapse, both the causes supposed to give a peculiarly unfavourable character to affections of the lungs would, so far as this lung is concerned, be destroyed. For in this situation the disused part would be placed in a quiescent state, receiving little or



JAMES CARSON (1772-1843)

no disturbance from the movements of respiration which would be performed solely by the other lung; and the divided surfaces would be brought into close contact by the same resilient power which before had kept them asunder."

Carson opened the thoracic cavity of a rabbit on the left side and saw that the lung became collapsed. The animal "lay for a few seconds as if it had been stunned by a blow on the head; it then leaped up, skipped about the apartment, took food and appeared in all respects as if nothing had been done to it" After about five days the contralateral right side was opened. The rabbit breathed short, rapidly and laboriously, was very restless and so weak that it could not stand upon its feet; reclining from right to left and changing position frequently. After about two hours, however, it began to recover gradually, to leap about, take food and breathe much more normally. After five days, with the exception of some interference with respiration, the animal seemed quite well. The animal was then killed. "The incisions upon both sides were closed though the sides of the wound which had been last made were easily separated. Animals (rabbits) in which the incision was made on both sides at once died immediately." From these experiments Carson drew conclusion that "there can be no doubt that one of the lungs of an animal may be reduced to a state of collapse with perfect impunity." He went on to consider why one lung could be collapsed without the other being affected. "In the case of the collapse of a single lung the impunity is no doubt secured by the tension of the mediastinum . . . which, after the collapsing of one lung, by the admission of external air into contact with its surface, secures to the other lung nearly its own proportion of the cavity of the chest and protects it from being materially impeded in the performance of its functions." He then turns to the treatment of lung lesions in the human being: "The next object is to consider the effects which the collapse of a single lung or of both lungs in succession is likely to produce in some of the diseases of the chest. . . . This argument may perhaps receive some illustration from considering what takes place when the tendon achilles is ruptured. In consequence of the elasticity of the tendon and

the retraction of the muscles of the leg, the ends of the divided tendon recede from each other and an union between them cannot be effected unless they are brought into and kept in contact by force."

"It not unfrequently and in the early stages perhaps generally happens that the deplorable disease termed consumption has its seat in one lung only; and when this occurs there is little difficulty in general in ascertaining to what lung it belongs. The means we possess of reducing this lung to a state of collapse or of divesting it for a time of its peculiar functions are equally simple and safe. In those cases in which the disease is placed in one of the lungs only the remedy would appear to be simple, safe and complete. . . . In those cases in which both lungs are affected greater objects are required to be surmounted. We have seen that when the external air is admitted into both pulmonary cavities at the same time by openings made either through the diaphragm or between the ribs, the death of the animal is the immediate consequence. We have, however, witnessed that if openings admitting the external air be made in succession, the last being after an interval of a few days from the first, the life of the animal, though exposed to the greatest danger, may be saved. A process, as had already been remarked, seems certainly to have been going forward, the termination of which would appear to be the restoration of the collapsed lung to its former dilated and efficient state. If the collapsed lung were expanded, as I think must be admitted to be the case to a certain extent, in the space of a few days, it is presumed that a greater degree of the same effect would be the result of a more lengthened period; and that it may be hoped that after the lapse of a given time, the operation may be performed on the second lung with as much safety as it had been upon the first."

Carson had read of cases recovering from consumption after the onset of spontaneous pneumothorax. In addition he had heard of wounded men recovering in the same way. He proceeds: "It is fully in my recollection to have read of cases of consumption having been cured by the chest being deeply wounded in battle. . . . The cure, both of the wound and of the previous

disease, depended upon the same cause, the reduction of the diseased and the wounded lung to a state of collapse. . . . In such a case it may be concluded that one of the lungs only was wounded for, if both had been wounded by a passage being made through the mediastinum, the accident must, I conceive, have been fatal." He speaks of abscesses of the lungs. "By the collapse of the lungs the cavity of the abscess must necessarily be destroyed. The walls of the abscess must upon the opening of the chest undergo a change similar to that experienced by the womb upon the delivery of a child and a haemorrhage from one of the lungs, the frequent prelude of consumption, if not immediately fatal, would certainly be stopped by the collapse of that lung as the flooding, consequent upon parturition by the contraction or rather the resilience of the womb. . . . In cases where the blood might cause suffocation by entering the trachea or the pus from an abscess in the same way" he recommends a modification in technique. "To obviate these dangers, the plain and simple means are to reduce the lungs thus situated to a state of collapse by degrees only. This might be accomplished by admitting a small quantity of air into the cavity of the chest at one time and allowing an interval to exist between the successive admissions which may be necessary before the lung shall be brought to a complete state of collapse." Of Consumption he says: "It crops the flower of the human race."

"It has long been my opinion that if ever this disease is cured, and it is an event of which I am by no means disposed to despair, it must be accomplished by mechanical means or, in other words, by a surgical operation."

There it is! The gift of prophecy of a kind that is based on sound experiment rather than mere speculation. In my opinion we ought to hold the name of James Carson very high in the annals of British medicine instead of allowing it, as our profession did, to pass to an unhonoured oblivion; to be forgotten.

Leaving the original, we may now once more quote Lawrenson Brown (1941) who gives valuable references to subsequent work of various authors referring to Carson. "John Davy,

stimulated possibly by Carson's work, published 'Observations on the Air found in the Pleura in a case of Pneumothorax with experiments on the Absorption of Different Kinds of Air introduced into the Pleura.''' (*Phil. Trans.* XXIX, p. 496, 1823). This paper which I have myself consulted described the collection of the gas from a case of spontaneous pneumothorax; its composition, namely, CO_2 , 8%, and N_2 , 92%, and the fact that there was no O_2 . This composition is quite in keeping with Sir William Ramsey's analysis of the gas taken from a pyo-pneumothorax (Morrison Davies, *Surgery of the Lung and Pleura*, 1919, p. 54). He noted also that there was, in nature, no air in the pleural cavity and he must have observed that the two layers of the pleura were held together by a negative pressure. There is nothing in his interesting paper, however, to suggest that he was aware of Carson's work, though the dates correspond. Carson's observations naturally met with some criticism. Brown quotes E. F. C. Sturdwick of Hillboro, N.C. as follows: "It becomes apparent that all that could be obtained from this operation is relief from motion which cannot rectify the vitiated state of the lungs productive of consumption." He concludes that "if this be so, the operation must be condemned." (Philadelphia, *J. Med. & Phyl. Soc.*, 1824.) There were more favourable opinions, however, though not many, about this time. Brown states that "F. H. Ramage, an Irish physician, is said to have used the method of artificial pneumothorax in one case at least, with excellent results for, at the autopsy (cause of death not known) about eleven months after the treatment was begun, the tuberculosis of the lungs was almost healed." This paper I have not been able to consult. It is not available in the library of the Royal Society of Medicine, and the library of the Royal College of Physicians is not, at present, reassembled after the war. It may be in some American journal, but no reference is given. In the *Special Report Series of the Medical Research Council*, No. 67, of 1922, L. S. T. Burrell and A. Salusbury MacNalty, in a paper referring to artificial pneumothorax, state that "Cayley treated a case of haemoptysis at the Middlesex Hospital by this means in 1885." They do not say with what result. Parker, in his paper in the

Lancet, No. 17, of April, 1882, was approaching the subject but not exactly on the same line when he wrote of "Treatment of Special Cases of Empyema by Thoracocentesis with the simultaneous Injection of Purified Air." Nor was Carson himself satisfied to leave to animal experiment alone a method which appeared so applicable to human beings. He tried it, or rather got it tried, he himself being present, on at least two human beings, though not with success in either. One was a merchant of Liverpool named James Sloane, the last of five brothers, the others having died of phthisis a few years before. Sloane had returned from a trip to the West Indies without experiencing the expected benefit from a change of air and climate. The operation was done, at his own request, "on September 26th, 1822, by Mr. Bickersteth, Surgeon, in the presence of Dr. McCarthy and myself. An incision calculated to admit air freely into the chest was made between the sixth and the seventh ribs. As the sound usually heard upon the opening being made into the chest and produced, no doubt, by the rapid passage of air through the opening, was not perceived in this case it was suspected that the lung did not collapse and that the adhesion prevented the entrance of air. It was not deemed advisable to make a further examination at the time." So says Carson and we must conclude that he was right. The operation should probably never have been performed on James Sloane whose phthisis appears to have been much too far advanced and too much complicated by adhesions between the visceral and the parietal pleura to give the method any chance of succeeding. We fear, too, that the open method, even if the case had been more suitable, might not have been sufficient. The introduction of sterile air into the intrapleural space by means of a needle, the graduation of the amount as shown on the manometer, the repetition at short intervals of the necessary refills! This sort of pneumothorax might have succeeded, if not in the case of James Sloane, at least in many other "early" patients. But the method of "open incision" into the thorax, thus allowing of as much to enter as happened to be pulled in by the collapse of the lung, the serious limitation of refills by the necessity of a further division of the chest wall on each occasion;

such a technique must have made the field for "open pneumothorax" very small. It was only very much later, and with no knowledge of Carson's previous experiments and conclusions, that Forlanini, in his first paper on the subject, "*Primi Tentativi di Pneumotorace Artificiale*," in the *Gazz. Med. di Torino* (May 17th, 1894, No. 20, p. 381), advocated the use of a needle. "He insists on a common Pravtz (large hypodermic) needle," says Salvatore Lojacono, in his summary of Forlanini's work in *Tubercle*, Nov. 1934, pp. 54-60. "Small amounts, 200 to 250 cc. of nitrogen and frequent refills, at first daily, then every two days, and later every week or at longer intervals." If, as seems probable, Forlanini was the first to use a needle and frequent refills, then he stands forth as the true parent of modern "artificial pneumothorax"; but the principle of it must, of necessity, be credited to one whose work had been entirely forgotten, to one who had seen the great vision of pulmonary collapse therapy; to Carson, in fact, who set out in 1822 to "consider the effects which the collapse of a single lung or of both lungs in succession is likely to produce in some of the diseases of the chest."

GEORGE BODINGTON (1799-1882)

IN George Bodington we have the very mirror of an English gentleman; sensible, gallant and gifted with a fine sense of language as proved by his admirably expressed though unfortunately short book on tuberculosis; and prepared to face martyrdom or enthusiastic recognition as a matter of course if only he might say openly that what he had himself *observed* in his tuberculous patients was literally true because he had seen it with his own eyes. Dr. James Gilchrist, himself a great authority on the disease, kindly told me of the survival of Dr. Bodington's grandson and gave me his address. The result has been a most interesting correspondence with Dr.

Arthur Bodington, one of whose letters gives the following information:

1 Kingsley Terrace,
Westward Ho
26.9.1946

Dear Sir,

. . . My grandfather was born on May 17th, 1799, and died on Feb. 5th, 1882. He was at Magdalen College School and sang in the Choir as a small boy. He was apprenticed to John Syer of Atherstone in 1816, which indenture was transferred to a Dr. Wheelright in London in 1818, which enabled him to attend St. Bartholomew's Hospital; one certificate of attendance was, I see, signed by the great surgeon, John Abernethy. He was qualified as L.S.A., and went into a practice in Erdington later taking Driffild House at Sutton Coldfield which was conducted by Messrs. Terry and Horton, surgeons, as a Private Asylum. In 1840 he sold his practice to a Dr. Moffat, retaining the Asylum, and took a house at Maney where he established his open air treatment for tubercular patients. This he gave up later and transferred his Asylum to his son, my father, in 1867, when he retired from practice altogether. He married Ann Fowler, in 1824 or 1825, of Gravelly Hill, Erdington, and had four sons and two daughters. He took an active part in Municipal affairs in Sutton and was Warden in 1852-1853. In his younger days he was a good cricketer and there was a little local doggerel poem—one verse of which only I have heard—which states that

“The Doctor came down from his house on the hill
As good with the bat as with physic and pill
The runs and the wickets he took all himself
And the poor little lame man was left on the shelf.”

I do not know who the “little lame man” was.

He assisted the Rector, Rev. Roland Bodford, in forming the Free Foresters' Club, which first began to play at Sutton Coldfield at the end of the 50's. He was also a fine shot and I remember him when I was a small boy coming over to my

father's house in Staffordshire to get rid of some moorhens which had overrun the pools there. He used to go out in a Governess Cart and with him two dogs, a setter (Bell) and a pointer put in the back, and shoot over his skilful dogs. He was, I believe, a good speaker and, at one time, offered himself, unsuccessfully, as a Protectionist to the electors of Birmingham. He died in 1882. I hope this will give you the information you need. I ought to have said that he was the son of Joseph Bodington of Kenilworth Chase, and a grandson of John B., of Kenilworth Chase, who succeeded to the property at Cubbington, including the Stone House Farm, which was still owned by Rev. Herbert James Bodington and one of his sons until about 20 years ago.

Yours truly,

Arthur E. Bodington

Doctor Arthur Bodington also adds a few family details. He says that his grandfather was the youngest of six brothers, all over six feet except Bodington himself who was, therefore, called "little George." One of them became a noted surgeon. Doctor Arthur tells of him that the Lady Edward Newdegate of Arberry, then an old lady, told him how she remembered his grand-uncle, the surgeon, as having an extensive practice and doing his distant rounds on horseback. "The Bodingtons of Cubbington Grange," he adds, "which property came into the family in 1556, were a yeoman family."

This brief account gives all the information that one ought to require for such a brief article as this about the life and work of a man devoted to the care of his patients rather than to other things. Still it is very pleasant to picture his setting off with his two dogs to shoot and enjoy himself when his practice allowed him. One might amplify the story of his ancestry by quoting the *Lancet* of March 11th, 1882, which said of him that he was "the descendant of one of the old yeoman families of Warwickshire, the Bodingtons of Cubbington, who had tilled their own land in that parish since the time of Henry VIII"; which agrees with his grandson's account. We have said, already, that he was the sort of man who would face martyrdom

or accept recognition at the hands of his profession with complete indifference. He got his martyrdom during his early life and most of his recognition after or shortly before his death. The acquisition of Driffield House Asylum must have made a considerable claim on his time but he continued to practise general medicine and the taking of the house at Maney must have given him the great pleasure of applying his own treatment to the tuberculous people who might come to him for his services. Here he worked his wonders and the result was the publication of his little book, *The Treatment and Cure of Pulmonary Consumption*, 1840, on the contents of which his reputation as the first person in England to think out and apply what we now call sanatorium treatment rests securely.

The appreciative obituary notice in the *Lancet* of 1882 contrasts sharply with what the *Lancet* wrote of Bodington's work in 1840. His challenging and revolutionary pamphlet, as it must have appeared to the average medical man at that date, was greeted as follows: "The modest and rational preface with which the Author introduces his pamphlet on pulmonary consumption has so far influenced us that we shall give an outline of his principles without expending any portion of our critical wrath on his very crude ideas and unsupported assertions. . . . The measures then which Mr. Bodington assures us have been 'uniformly and completely successful' are detailed in the following pages: *vide* p. 13, line 12 to p. 18, line 1. More agreeable and seductive medicaments could not certainly be found in any pharmacopea. Fresh morning air to make the patient breathe; good wine to bring down his pulse, a good dinner to make him fat and an opium pill to make him sleep, are all excellent remedies if they would only have the desired effect. . . . Dr. Bodington informs us that in the dry sharp air of Warwickshire they have marvellous effects and we are bound to believe him although we should have desired better evidence than is furnished by the following cases: A 'young man about nineteen years of age, after having a year before suffered from haemoptysis severely and subsequently from slighter attacks of that disease from time to time, became the subject of a very severe hypocondriacal affection which, in

the month of August, 1839, terminated in the development of tubercular consumption, characterized by frequent cough, and expectoration of mucus and pus, or matter of an ashy colour, sinking in water; by nocturnal profuse perspiration, shortness of breathing, emaciation and great debility; pulse ranging 130 to 150 in a minute, respiratory murmur almost imperceptible; percussion over the clavicles gave a dull sound; internal resonance of the voice and cough on the right side; the whole symptoms, physical and natural clearly demonstrating the existence of ulceration or excavation of a portion of the lungs, constituting the last stages of consumption.' If these symptoms, physical and natural, clearly demonstrate the existence of ulceration (?) of the lungs, or, in other words constitute the last stages of consumption, then has Mr. Bodington proved his case and is entitled to national rewards equal, nay, superior, to those conferred on the illustrious Jenner."

As one reads the account of the case and its symptoms and physical signs one cannot but be impressed by the complete ignorance of the *Lancet* reviewer of all the factors taken into account nowadays in the clinical diagnosis of pulmonary consumption; this patient must have been indeed very far advanced as Bodington rightly surmised.

However, in the years that followed, the *Lancet* learnt to treat Bodington's views with more respect. As will be noticed this paper gave him a very appreciative obituary notice. "It is remarkable that a village doctor should have arrived in 1840 at these conclusions which anticipated some of our most recent teaching. It is less remarkable that he met with the usual fate of those that question authority." (*Lancet*, 1882.) But what was it that the *Lancet* found so "crude" in the passages which it quoted? Let us examine Bodington's methods once again to see, if possible, what led to this severe criticism. "In a consumptive patient we shall find first of all a rapid and weak pulse ranging from 120 to 140 beats a minute. . . . This condition must be met at once, not with antiphlogistics, but with frequent supplies, in moderate quantities of nourishing diet and wine; a glass of good sherry or madeira in the forenoon with an egg, another glass of good wine after dinner, and

nourishing food for supper, etc. I have generally succeeded in the course of a few days in reducing the pulse from 130 to 140 down to 90 and, as to sedatives, the direct or full dose is given at bed-time to allay coughing and procure sleep." Advice such as this may have appeared "crude" to a generation which placed its consumptives on a greatly reduced diet, stopped their activities, told them, in so many words, that they were bound to die rapidly from a fatal disease and advised them to go to Switzerland or the sea-side or anywhere to add the advantages of healthy surroundings to their last miserable days. But who is there that will advise against a good diet for the consumptives today; against a glass of wine, if they can afford it; against a dose of opium at night if they require it? This advice to a consumptive is not "crude," what ever else it may be. Or was it "crude" to advise against air and exercise? "I come now to the important remedial agent in the cure of consumption," says Bodington, "that of the free use of a pure atmosphere . . . the air out of doors early in the morning either by riding or walking . . . with intervals of walking as much as the strength will allow of, gradually increasing the length of the walk until it can be maintained easily several hours a day." Why, this is nothing but an excellent epitome of what, apart from operative treatment, we do today at every sanatorium! It is the "fresh air and graded exercise" which we all advise and which has done such wonders to early cases of the disease. "The abode of the patient should be in an airy house in the country; if on an eminence so much the better; the neighbourhood should be dry and high; the soil a light loam, a sandy or gravelly bottom; the atmosphere is, in such situations, comparatively free from fog and dampness. Thus the equal temperature so much considered and said to be necessary should be that of the external air instead of that so commonly employed, the warmth of a closed room. . . . In those cases which I have treated upon these principles having had some of the patients under my own roof, by which I secured all the advantages of situation, etc., before spoken of, and some in my immediate neighbourhood so that I could closely watch them, I have met with signal success. . . . The generality of the medical profession have not the opportunity

of thus treating their consumptive patients; if they are to succeed they should have country houses in proper situations, well ventilated and provided with all appliances and means to boot where their patients should be under their own eyes and strictly watched and regulated in all respects as regards exercise, air, diet, medicine, etc., or there should be a certain class of practitioners who should exclusively pursue this practice as a distinct branch to whom those in large towns should confide their consumptive patients instead of sending them, as many do now, to take their chance or probably fall into the hands of mercenaries at some distant seaport where they commonly die, far away from friends and home." He adds that he has, to carry out this treatment, "taken for the purpose a house in every respect adapted and near my own residence, for the reception of patients of this class who may be desirous or who are recommended to remove from their homes for the benefit of a change of air." This may have appeared "crude" to the reviewer for the *Lancet* in those days; must have seemed "crude" to the average medical man reading the professional papers at that time; it is nothing less than an eloquent appeal for what we have—not in sufficient numbers, alas—in the sanatoria of today.

Thus far we have only mentioned the *practice* of George Bodington; and, after all, this is the part of a man's life that counts. The errors in theoretical conception were only what might be expected, having regard to the existing knowledge of the cause of tuberculosis which was his only guide. The good in his practice is mixed up intimately with the bad in his theory and it is naturally the good and the practical which survives him in the memory of medical men. Contrast the following passages: "I should recommend to one thus consuming away under the influence of this *wasting disease* a nutritious diet of mild, fresh animal and farinaceous food, aided by the stimulus of a proper quantity of wine, having regard to the general state and condition of the patient"; or the following: "The only gas fit for the lungs is the pure atmosphere freely administered without fear." Nothing could be better. The most modern ideas about tuberculosis cannot improve on these concepts

or put them forward in a more attractive shape. On the other hand, we quote the following: "Those persons who are for the most part freest from the attacks of consumption, such as agricultural labourers, are commonly but little troubled by this nervous disorder; they are rather remarkable for an apparent obtuseness of nervous susceptibility and this is in strict keeping with fully developed muscular, nutrient, and sanguiferous powers. . . . It is a plain inference that, to guard against the attacks of consumption, the condition of the patients should be assimilated as much as possible to that of the above-named class of individuals." Bodington did not and, perhaps, could not realize that the agricultural labourer, once taken out of his relatively bacterium-free environment and transferred to the germ-laden atmosphere of a large town, was much more susceptible to tuberculosis than his town-bred fellow. "Fully developed muscular, nutrient, and sanguiferous powers" have very little to do with resistance to tuberculosis. Acquired immunity or the absence of it is the really important thing. This part of the picture was as yet hidden from the eyes of the profession and was only to arrive with the germ theory of the disease. And yet the clinical advice to the patient could not be better: "The condition of the patients should be assimilated as much as possible to that of the above-named class of individuals." Though the idea that the nervous and muscular mechanisms of the tracheal and bronchial structures and of the lungs have supreme control of the power to resist tuberculosis is not true, it is, nevertheless, a *fact* that the same conditions which lead to a diminished charge of bacteria in the air lead also to a greater chance of recovery to the person stricken with tuberculosis; no one can deny that many such patients are all the better for a quiet life in the country away from crowds and with the advantages of pure air and sunshine added.

Then, again, the six patients mentioned as being almost at once cured or greatly improved by the treatment advised and administered by Bodington were obviously of a type which might have been better understood after a thorough and searching examination by strictly modern means, including X-rays. One of them subsequently died of tuberculosis after having

suffered from severe haemoptysis and the usual symptoms of the disease. We can count him as a very bad case whose fate was sealed before ever he took Bodington's treatment. He would have died either with or without modern sanatorium treatment. That leaves five to be critically examined. The first was an awl-grinder with a very bad history, one of his sisters having died at the age of about twenty of what was obviously tuberculosis. "His finger nails were incurvated; he was troubled with a pain in his side; and a cough more or less without intermission." The finger nails are much more affected in cases of abscess of the lungs than in tuberculosis. We read that "a feeling of suffocation affected him which was distressing, arising from an abscess in the bronchial passages" which was relieved by the bursting of the abscess. He was advised to take walking exercise and the usual sedatives and, *in a week*, he became convalescent, "losing his cough entirely." He resumed his trade as an awl-grinder and "continues it to this time." Now this man, although he had a bad history and probable contact with the tubercle bacillus, was still an awl-grinder and an abscess case. He had, probably, a certain amount of silicosis complicated with an abscess of the lung which, being healed in the usual way, by bursting, allowed his symptoms to abate and, under the sensible treatment recommended, to die away. Another of the series, Mrs. L., had severe symptoms and was very ill. "Eight or ten leeches were quickly applied to the lower part of the neck, just above the sternum; and shortly after their application, her mouth became suddenly filled with matter of a purulent character. . . . A large bronchial abscess had been the cause of the symptoms, and its bursting afforded the relief which the patient felt." The patient did very well! "In one week's time I met her riding several miles from home." Bodington adds that "the bronchial abscess was probably the result of tuberculous deposit and the case altogether a specimen of the acute form of consumption," but we know better nowadays. The removal of these abscess cases leaves us with only three remaining. But these three seem to have been genuine cases of consumption and to have responded exceedingly well to Bodington's treatment; so well, in fact, as to demonstrate

its great value. And there were probably many others that did remarkably well also; as well as cases that did very poorly; for we know that sanatorium treatment, even under the best conditions, must often fail. But it remains true that Bodington was the first to drive home the lesson that the most important requisite for the consumptive is "the free use of a pure atmosphere; not the impure air of a closed room or even that of the house generally but the air out of doors, early in the morning, either by riding or by walking." "The patient," he says, "ought never to be deterred by the state of the weather from exercise in the open air; if wet and rainy, a covered vehicle should be employed with open windows. The cold is never too severe for the consumptive in this climate; the cooler the air which passes into the lungs, the greater will be the benefit the patient will derive." And again: "How little does the plan of shutting up the patients in close rooms accord with this simple and obvious principle."

And so he started a separate *house* for his tuberculous patients; a house under his own supervision where the cases could have fresh air, good surroundings, suitable exercise, adequate treatment; a sanatorium, in fact, of a truly British kind, and devised long before Goerbersdorf or Falkenstein of any of the German places so rightly prominent in the story of sanatoria were thought of.

In other words, Bodington not only saw a vision but actually put the means thereof into practice. He was naturally—for all innovators suffer this fate at first—howled down by the medical profession of his day; but he now stands the originator of the system which at last, with the necessary adjuncts, is bringing the curse of tuberculosis under control.

WILLIAM BUDD (1811-1880)

WILLIAM BUDD was a man to whom the appearances, symptoms and character of pulmonary phthisis were well known, for he had been for a long time in practice at Clifton and Bristol to which places many phthisics repaired and to which, in an especial degree, the negroes employed on our ships came for treatment when they found themselves unable to continue work owing to the ravages of pulmonary consumption. He was therefore in a very good position to compare the chronic phthisis of Europeans and the swift and usually fatal form occurring in the blacks. We may imagine that he had meditated often on the differences between these two types and had been greatly puzzled by them yet without a clue until that great day when enlightenment came to him. "The idea that phthisis is a self-propagating zymotic disease and that all the leading phenomena of its distribution may be explained by supposing that it is disseminated through specific germs contained in the tuberculous matter cast off by persons already suffering from the disease *first came into my mind unbidden, so to speak, while I was walking on the Observatory Hill at Clifton in the second week of August 1856.*" With this idea came the sequel: "The Geographical distribution of phthisis in past and present times and especially its great fatality in countries which, when first discovered (by Europeans) were absolutely free from it. . . . When the South Sea Islands were first discovered phthisis did not exist there," and it was now very prevalent. . . . "Now, everywhere along the African sea-board, where the blacks have come into contact and intimate relations with the whites, phthisis causes a large mortality among them. In the interior, where intercourse with Europeans has been limited to casual contact . . . there is reason to believe that phthisis does not exist." This observation—and how true it was—came into his mind at about the same time that it entered the mind of Villemin, that great Frenchman who was to demonstrate by the

experimental method upon the bodies of rabbits and guinea-pigs the literal truth of it. How came it that, in the Englishman, it went no further than an illuminating idea? Budd was a general practitioner with all his notions held in check by the exigencies of his patients. Villemin, on the other hand, was a Professor at the French Army Medical College at the Vâl de Grace, with great opportunities for research at his disposal. In both there was the same rich and productive brooding over a common and terrible disease seen with eyes stripped of all convention; in both, there was the same response, an intuitive motion towards the truth. It was as if two duelling pistols had been prepared and only one loaded! When the triggers were pulled in both there was the flash in the pan. In the unloaded, the matter stopped there. In the loaded the charge was exploded and the bullet sent to fell its victim—the swollen bulk of authoritarian theory which was holding the minds of the profession in thrall! The result, then, was different. In the one case it eventuated in a letter to the *Lancet*¹—which was soon forgotten! In the other the theory of a microbic origin for phthisis was converted into the established fact that caseous material from the human, or better the bovine, could produce in the rabbit, a wasting and often fatal disease which was capable of being transmitted through animal after animal with the regularity of clockwork. It was reserved for Robert Koch to demonstrate the essential cause twenty years later and to focus the activity of all laboratory workers on the acid-fast bacillus which is the cause of tuberculosis.

¹ "Memorandum on the Nature and the Mode of Propagation of Phthisis." *Lancet*, p. 452, October 12, 1867.

PART II

The Early Continental Physiologists

THE EARLY CONTINENTAL PHTHYSIOLOGISTS

THE finding that one per cent or more of *persons in health* are suffering from early, radioscopically visible, lung lesions has so magnified the prestige of X-ray examination that it might be thought that the percussion of Auenbrugger and the auscultation of Laënnec were no longer of more than historical interest. This is not so, however, and a glance at the activities of the medical profession today will suffice to convince the observer that percussion and the stethoscope still represent the chief armamentarium of the average physician.

It may, perhaps, be forgiven me if I attempt to deal with the discoverers of these two methods in a historical treatise. History often throws a light on present-day methods and a fresh study of the works of Auenbrugger and of Laënnec as well as of their comrades and their opponents appears appropriate.

LEOPOLD AUENBRUGGER (1722-1809)

It seems proper that I should put Auenbrugger first among the continental phthisiologists, although he was never very celebrated for a study of phthisis, for he introduced what has proved to be one of the most fruitful methods for investigating diseases of the chest—percussion.

As the son of an inn-keeper father and a musical mother, he had the right physical make-up for the differentiation of sounds and must, as has often been pointed out, have seen his father investigating the amount of fluid in barrels by tapping their sides. He was suitably endowed, therefore, to appreciate the advantages of examining the human chest by percussion. Auenbrugger mentions this in Obs. III, Sec. 17 of his *Inventum Novum*. "And the Cause which occasions the diminution, whether solid or liquid, produces analogous results to those observed by striking a cask, for example, in different degrees of emptiness or fullness." But, with the passage of almost seventeen centuries of the Christian era, doctors still left the chests unexamined and depended upon cough, fever and wasting to indicate disease. It was still reserved for Auenbrugger to discover percussion. From Graz where he had been born he was launched by his father on the study of medicine and soon became a well-known figure in Vienna. He was a student under Van Swieten, the celebrated Professor of Medicine who had been attracted from Leyden to the Vienna school. In 1751 he became a physician to the Spanish Military Hospital and continued there for ten years, for the last seven of which he devoted himself to the study of chest-percussion. He published his *Inventum Novum in percussione thoracis humani ut signo abstrusos interim pectoris morbos detegendi* in 1761 when he gave up his connection with the hospital. "In making public my discoveries respecting the matter," he writes, "I have been actuated neither by an itch for writing nor a fondness for speculation but by the desire of submitting to my brethren the fruits of

seven years' observation and reflection. In doing so I have not been unconscious of the dangers I must encounter since it has always been the fate of those who have illustrated or improved the Arts and Sciences by their discoveries to be beset by envy, hatred, malice, detraction and calumny."

He was right. Though popular as a man and taken into favour by the Court and the Empress Marie Thérèse his *Inventum Novum* remained unnoticed even by those great physicians and professors, Baron van Swieten and his successor, de Haen. There must, however, have been some temporary professional interest in the matter for the little book went through two editions and Stoll, the successor to de Haen, rather favoured it. Albrecht von Haller thought the technique "worthy of close attention." Though quickly forgotten in Vienna and the surrounding countries, the *Inventum Novum* caught the attention of Rosière de la Chassagne of Montpellier in 1770 though this observer merely regarded it as "un moyen de plus qu'on peut employer sans risque," never tried it himself and confounded it with the Hippocratic succussion from which, needless to say, it differs completely. From that time percussion seems to have been completely ignored until it was found by Corvisart to be mentioned in the *Aphorisms* of Stoll and was republished by him in 1808, a year before Auenbrugger's death. The volume by Corvisart, voluminous as compared with the original seventy-four pages of Auenbrugger, consists of an elaborate analysis of the Sections of the *Inventum Novum*. It made the method widely known and gave it its place as a really valuable technique to be followed in the examination of the chest. Above all, it came to the notice of Corvisart's great pupil, Laënnec, and thus ensured its combination with mediate auscultation as a most promising procedure.

Up to the time of Corvisart, and for several years after, percussion meant the simple tapping of the chest with the fingers worked from the wrist, *not* from the elbow or shoulder. "The thorax," he says, "ought to be struck, slowly and gently with the points of the fingers, brought close together, and at the same time extended." Corvisart adds that "Percussion with the flat of the open hand is also useful, especially

in the lateral and posterior parts, in ascertaining the precise extent of the cause obstructing the sound." (Commentary to Obs. II.) This method, not very useful, I fear, is the first modification suggested to the Auenbrugger technique. In 1828, Piorry introduced the method of "percussion mediate," the use of a pleximeter; that finally preferred by him was a plate of ivory, to be held against the chest with a bent handle. He tapped upon this with the finger. This great addition appears to have been accepted as a mere amplification of Auenbrugger's percussion technique but it was in reality a valuable extension of it and deserves to be particularly noted. Piorry mentions that a few British and American doctors visiting his clinic to be instructed in the new method—they are not even named—tried percussion on a finger of the left hand applied to the chest as a pleximeter but he takes the view that this cannot be compared to the ivory pleximeter for the examination of the chest. Here, I think, Piorry is on doubtful ground. The finger, always available, is the best pleximeter of all. I wonder who it was that first tried this modification. It is certain that he introduced a wonderful simplification of the method. There is, I think, a sensation to be gained with the finger-pleximeter itself which lends a definite precision to the method of percussion; a sensation which is to a great extent lost with the ivory instrument merely because it is a detached instrument instead of a natural part. This, however, is an individual opinion and must be taken as such though I feel sure that many will agree. These modifications are of great importance and have amplified the method for general use.

But above all there remains the wonderful observation of Auenbrugger that the thorax, on being tapped, has, even in health, several voices and that these may be further modified in disease "higher, deeper, clearer, more obscure or almost suffocated." "I here present the reader," he says, "with a new sign which I have discovered for detecting diseases of the Chest." Let us first confine our attention to Auenbrugger's description of the normal chest.

"The thorax of a normal person sounds when struck.

The sound elicited from a healthy chest resembles the

stifled sound of a drum covered with a thick woollen cloth or other envelope.

This sound is perceptible on different parts of the chest in the following manner:

On the right side anteriorly it is observed from the clavicle to the sixth rib; laterally from the axilla to the seventh rib; and posteriorly from the scapula to the second and third false ribs.

The left side yields this sound from the clavicle to the fourth true rib anteriorly; and on the back and laterally in the same extent as on the other side; over the space occupied by the heart the sound loses part of its usual clearness and becomes dull.

The whole sternum yields as distinct a sound as the sides of the chest except in the cardiac region where it is somewhat duller.

The same sound is perceptible over that part of the spinal column which contributes to form the chest.

Any interference with these sounds by consolidation or excavation of the lungs or by fluid or air in the thorax must be taken as evidence of disease."

Auenbrugger's description of the sounds to be elicited by percussion in disease is even more striking—or just as striking—as the above. In his *Obs. III.* and the succeeding *Scholium*, he says: "Of the preternatural or morbid sound of the Chest and its general import . . . To be able justly to appreciate the value of the various sounds elicited from the chest in cases of diseases it is necessary to have learnt by experience on many subjects, the modifications of sound, general or partial, produced by the habit of body, natural conformation as to the scapulae, mammae, the heart, the capacity of the thorax, the degree of fleshiness, fatness, etc.; in as much as these various circumstances modify the sound very considerably. . . . If, then, a distinct sound, equal on both sides, and commensurate to the degree of percussion, is not obtained from the sonorous regions above mentioned, a morbid condition of some of the parts within the chest is indicated." This may seem to some of us, taught, as we are to give its true value to percussion, to be even too simple a

warning, but we must remember that Auenbrugger was dealing with a new subject full of possible errors to the beginner and to be approached with due precaution. And, as a matter of fact, one has only to observe a class of students dealing with percussion for the first time to appreciate how necessary the warning is as to "the natural conformation of the chest," etc.

In Scholium XVI of the same Observation he says: "If a place naturally sonorous and now sounding only as a piece of board when struck still retains the same sound, on percussion, when the breath is held after a deep inspiration—we are to conclude that the disease extends deep into the cavity of the chest." After repeating the Scholium already quoted, as to the striking of a cask, the commentator Corvisart decides to quote from Laënnec, choosing a passage from *Mediate Auscultation* which we may venture to repeat here as showing how the latter selected points from his predecessor and improved on them by his own extremely vivid observations: "In the first place, percussion requires that we compare the same points precisely on both sides of the chest. For instance, if we have percussed on a rib on the one side, we must be careful not to percuss on an intercostal space on the other; we must likewise be careful that the skin and muscles are equally tense on both sides, which is often not readily effected when the patient is lean. It is still more essential to use the same degree of force on each side and to strike with the same degree of perpendicularity; for even a slight degree of obliquity in the stroke will diminish the intensity of the sound one half—a circumstance which might lead to great mistakes." In the Sixth Observation, Scholium XXVI, Auenbrugger asserts that "the preternatural sound observed in chronic diseases is owing either to—(1) some hidden condition of the organs which disorders them with a slow progress and finally destroys them; or exists (2) when certain obvious causes have induced a slow disorganization of the same." Under the first class he puts (1) those that depend upon a hereditary predisposition; (2) those which arise from affections of the mind, particularly ungratified desires, the principal of which is *Nostalgia*; "(3) those which affect certain artisans, naturally possessing weak lungs." Those who are so busy

nowadays in searching the sanatoria for psychological phenomena will, doubtless, be delighted at this early reference to nostalgia, a condition which may or may not be important in the average tuberculous case; but the inclusion of "certain artisans" is of real interest to workers in this field; especially when he goes on to say: "Our particular business, at present, is with those arts which dispose to diseases of the chest indicated by the sound so often described. Thus I have remarked that Tailors, Millers, &c. who are forced to inhale, during their labours, a fine dust, become phthisical." Auenbrugger had got on to the facts and even to an explanation of them, aided by nothing but percussion! As he says in Scholium XXIX, "For the above reasons it may be received as a general rule in chronic diseases that when, together with the indication stated in XXVI, there are emaciation and debility—the case is desperate." He goes on to discuss empyema and "dropsy of one side of the chest" with the same skill and, be it remarked, he notices the increased sound above the dullness when the fluid only fills part of the thorax and the alteration of the dullness with the position of the patient. "If the chest is only half filled, a louder sound will be obtained over the parts to which the fluid does not extend; and, in this case, the resonance will be found to vary according to the position of the patient and the consequent level which the liquid attains."

"What I have written," says Auenbrugger, "I have proved again and again by the testimony of my own senses and amid laborious and tedious exertions; still guarding, on all occasions, against the seductive influence of self-love."

Before bidding adieu to Edler von Auenbrugg—he was ennobled in 1784—it is but fitting to pause a moment and to pay a tribute to the memory of a man who, steadily and quietly and in the face of considerable opposition and ridicule, continued to study chest diseases by his "novum inventum," percussion, and with results of the greatest magnitude which were only to be appreciated years after his death.

Auenbrugger must have had a very well-developed musical side which, however, tends to be forgotten when compared with his wonderful discovery of percussion. He was thoroughly

popular with those about the Court at Vienna and it seems that it was at the instance of Maria Theresa and the Emperor, Joseph II, that he wrote his opera, *The Chimney Sweep*, a work which must have been far easier than the writing of the *Inventum Novum* but which was equally—and permanently—forgotten during the ensuing years.

JEAN NICHOLAS CORVISART (1755-1820)

CORVISART was originally destined for the legal profession and, as a lad, was taken into the office of his father, who was a solicitor to the Crown, with that end in view. But he did not, apparently, feel very happy in the occupation which had been chosen for him and was inclined to wander from his proper work into the Hôtel Dieu in the Latin Quarter, there to listen with rapt attention to the lectures and the clinical teaching of Desault, at that time the genius of the hospital. Finally the call to medicine became too ardent to be resisted and Corvisart entered as a medical student, being called as a Doctor in 1782 under Desault himself. At first he worked as a parish physician at a very small salary and, when a vacancy occurred at the Hôpital Necker, he very naturally applied for it. His qualifications and recommendations were examined and found satisfactory, but in the event everything turned, we are told, on whether he would be prepared to wear a wig while attending at the hospital. History says that he was very decided not to wear any such adornment and wrote a letter declining the appointment with the remark that "respect for outward signs must not degenerate into superstition." In 1787 he became physician to the Charité and, after a little time and a great deal of teaching, was advanced to be the medical director of that institution. In 1797 he was appointed Professor of Practical Medicine at the Collège de France and, ten years later, was promoted to be Physician-in-Ordinary to Napoleon Bonaparte



(J. N. CORVISART)
*Premier Médecin de S. M. l'Empereur & Roy,
 Officier de la Légion d'Honneur, Bureau de l'Empire, &c. &c.*

JEAN NICHOLAS CORVISART (1755-1820)

who is said to have professed "no faith in medicine but great faith in Corvisart." Among his pupils were Laënnec, Bayle and Dupuytren.

It was in 1797 that he became interested in Auenbrugger's work, till then quite forgotten. Sigerst, in his valuable book, *Great Doctors* (London, George Allen & Unwin, Ltd., Museum Street, 1937) speaks of him as follows: "Now he was a clinician but there was no clinical tradition in France. He therefore determined to get into touch with the Viennese school since the Vienna clinic had set an example to the world." Of the three books he wrote, two were translations of the works of Viennese physicians. He studied the writings of Stoll and found therein the *Aphorisms*, one of which contained Auenbrugger's *Inventum Novum*, a method of approach to chest examination which he had never heard of in Paris. "Percussion made a strong impression on me," he says, "and I think that since then I have practised it unceasingly whether in obscure diseases of the chest or in those that were simple and easily understood. It never led me astray if the conditions was such as to allow me to make full use of it." Thus speaks Corvisart and it is easy to understand why a clinician, once aware of this great and, we may almost say, indispensable method should not merely adopt it for his own use but should determine to make a translation of the *Inventum Novum* for the instruction of others and as the basis of his *Commentaries* which are so wisely written and which follow each section and "scholium" of the work.

Not that Corvisart's translation was the first to appear in the French language. In 1770, Rosière de Chassagne had also made a translation; he did not, however, try the method himself or recommend it very strongly to others. He thought it, as he says in his publication, as "un moyen qu'on peut employer sans risque" and, as such, translated it into his own language although confusing it with the Hippocratic method of "succussion" from which, of course, it is quite different. But he failed to impress the profession, already immersed in its own concerns, and his effort seems to have been but little noted and soon forgotten. He must, however, have found it interesting and valuable or he would never have gone to the trouble of

translating it and publishing it as part of his book! But it needed the dynamic personality of Corvisart to size upon the method—it is probable that he had never heard of Rosière de Chassagne or his writings—translate anew and publish it to the world with his striking and interesting commentaries so that it might, in due course become universally known and universally practised as part of the basic knowledge of the medical profession. Corvisart, then, must be acknowledged as the real transmitter, the true “bridge,” between Auenbrugger’s work, the *Aphorisms* of Stoll and the great medical public of France and, later, of the world.

But not only did Corvisart make known the work of Auenbrugger after it had been forgotten for thirty years! He was also the teacher of a still greater magician—if I may use the word to describe a marvellous pathologist and clinician—a greater than either Auenbrugger or than himself.

Laënnec, filled with the fire of original thought, was his pupil from the beginning of the nineteenth century. Even as early as 1803 he had published his first contribution to knowledge in the *Journal de Médecine*, Corvisart’s own journal; “Observation sur une Maladie du Cœur (Ossification de la Valvule mitrale, dilation du Ventricule droit) avec affection du poumon et de la plevre gauche,” thus showing his vivid interest in cardiac and pulmonary disease. Corvisart, then, must be admitted as giving the first stimulus to Laënnec and thus, in part at least, his own contribution to what was to be the outcome of Laënnec’s genius, *auscultation médiate* and the stethoscope.

Corvisart, as has already been recounted, was appointed Physician-in-Ordinary to Napoleon, and was created a Baron which added proportionally to his social duties and to his practice. The year 1815, marked by the escape of Napoleon from Elba and his final defeat at Waterloo, was a sad one also for Corvisart, for he was obliged, by an attack of apoplexy, to abandon his work and retire to his country place, where he died in 1820. He was, in the best sense of the term, what Sigerst calls him, a Great Doctor.

GASPARD LAURENT BAYLE (1774-1816)

BAYLE was born in August, 1774, at Vernet, a village in the high part of Provence in the department of the Basses Alpes. His family had come originally from the valley of the Ours in Haut-Dauphiné and had settled in the valley towards the middle of the sixteenth century. Most of its members had followed the profession of arms or had been occupied as lawyers. The family was pretty well off, property having been acquired, especially by a certain Jesse Bayle, a captain of Cuirassiers in the army of Henry IV. Bayle's father had been a lawyer to the parliament of Aix but he abandoned his career as a barrister early and devoted himself exclusively to the claims of his numerous children and to the administration of his patrimony. This information is taken from the short account of Bayle by his nephew, A. L. J. Bayle, written as a foreword to the posthumous work, *Traité des Maladies Cancéreuse* by G. L. Bayle, and published by M. Laurent, Paris, Rue Servandole, in 1833. Gaspard Laurent, as a schoolboy, showed great intelligence and a marked interest in the study of nature, insects in particular, and probably other living things as well. At twelve years old he was sent to the College of Embrun where he was particularly noticed by Father Rossignol, a Jesuit and the Principal of the College, celebrated for his opposition to the great naturalist, Buffon. Rossignol took him to live with him and taught him much in the way of natural history and of mathematics; they kept in touch with each other by letters until the death of Rossignol in 1813.

Bayle first studied for the Church and very nearly took Orders but finally abandoned it for reasons of conscience, not considering himself fit for such a career. He returned home to consult with his father and brothers as to what he should do and finally decided to become a lawyer and, as a start, to work in the office of his brother, Charles Bayle, one of the administrators of the Directory of the Department. All these events

took place in the year 1793, a year of great struggle and disturbance in France. Louis XVI had been executed in January of that year; France was menaced from her frontiers; the terrible Vendean insurrection had broken out in March and was still to continue with its bloody reprisals and counter-reprisals for many months to come; the Mountain was all-powerful and the country in the hands of the "terror." Bayle himself, although only nineteen years old, was already a member of the tribune of the Société Montagnarde d'Embrun where he made such a remarkable speech on the duties of a true republican and the unfortunate direction which was being impressed on the society that the latter got rid of some of its more advanced members who had latterly pressed it onwards to support the "terror" and all its works. A further proof of the courage of Bayle was given when Barrés and Fréron were sent by the Convention in Paris into the provinces of the Midi to execute the bloody decrees of that assembly. He spoke out freely and convinced the Proconsuls that they had better, at present, leave the Department alone. They went on with their journey without doing anything locally but, meanwhile, declared that the man who had spoken so vigorously against their policy should be arrested. Bayle, thus threatened with immediate reprisals, was persuaded by his father and brothers to go away at once, to seek refuge at Montpellier, and there to take up the study of medicine at the university. He flew during the night with all the haste the situation demanded and was only just in time as he was sought for early the next day.

Bayle was thus, as it were, accidentally thrust into the pursuit of what, to a man of his type of mind, was one of the hardest and most exacting of professions; a calling which was hardly less than the devotion of all his life and all his energies to the good of others. He had been utterly absorbed in poetry. He decided that this must be abandoned and, in his new ardour, burned all his cherished writings. "Entièrement guéri de sa préoccupation poétique Bayle se livra avec ardeur à l'étude des divers branches de la médecine." Thus writes his nephew and indeed we can sympathize, though not approach his height of self-sacrifice. There was one side of his education, however,



Gaspard Laurent Bayle.

Né le 15 Août 1774, mort le 11 Mai 1816

GASPARD LAURENT BAYLE (1774-1816)

which he still thought equally or more important than his medical career. He was, at this time, greatly interested in religion and reserved to its study some at least of the energy that was in him. He read the works of Diderot and of Voltaire as enemies of the faith which he was anxious to vindicate—and was, on the contrary, won over by them and “converted” from his previously unrestricted beliefs! Further study was, however, to restore him to a quiet belief in the Catholic faith which he never afterwards abandoned. After three years at Montpellier Bayle was sent to the army as a medical officer, being first attached to a military hospital at Barcelonnette, and then to another at Nice where he was under the command of Professor Desgenettes, the *médecin-en-chef* of the armies of the Midi. This turned him to clinical studies and he devoted himself to hospital work and, as a development of it, to pathology. He went to Paris in 1798 and from the time of his arrival there took many courses, much trouble, and every possible opportunity to study and aim at perfection in pathology and medicine. At that time Corvisart, continuing the work of Bonet and of Morgagni, inspired many of the students with this aim and Bayle amongst the number. He was fortunate enough to be nominated as an *aide d'anatomie* and redoubled his efforts to excel. “En effet, à peine était-il nommé à cette place qu'il se mit à faire l'ouverture des cadavres qui étaient déposés en si grand nombre dans les pavillons de dissection de la Faculté de Médecine à Paris.” He was nominated a Doctor of Medicine in 1802. In sustaining his thesis on that occasion he had an argument with Petit-Radel on the point as to whether “white tumours” were to be regarded as “inflammatory” or not. Here we scent the influence of Broussais who was attempting to prove everything, even tuberculosis, to be of inflammatory origin. Bayle said in reply: “Il est facile de l'attribuer à la bile ou à toute autre humeur à laquelle on accorde un degré d'acrimonie convenable; on peut mettre tout cela dans un beau livre [here he was having a hit at Petit-Radel who had just published one], mais il n'est pas aussi aisé de le connaître que de l'assurer; tout cela n'est fondé que sur des hypothèses et l'on doit sans doute abandonner le plus beau génie

lorsq'il veut, dans le délire de son enthousiasme, fonder sur l'imagination l'explications des opérations de la nature; car on observe la nature; on ne la divine pas." We return to this later.

Having been received Doctor of Medicine, Bayle obtained, by means of competition, the appointment of "Internal Student at the Hôpital de la Charité" where Corvisart and Dumangin were the doctors employed at that time. Bayle's work there, in such renowned company, gave him, in the words of his nephew, "that extraordinary medical tact and that accuracy in prognostication so rare even among the most celebrated practitioners." This he gained or added to by writing with the greatest care "the histories of all the individual cases of illness that presented themselves without distinction, in indicating the probable issue, in anticipating for those that appeared incurable the organic changes that one ought to find after death, and in comparing these prognostications with the event." These records were available to him for quotation later. "Bayle owed much to the invaluable lessons of Corvisart which contributed a great deal to his industry in following his taste for pathological anatomy." In 1805, Bayle was appointed at Dumangin's request, as a temporary medical officer to the Charité and was confirmed as a permanent member of the staff in 1807. There he practised and there he was recognized as one of the most consummate of doctors by such men as Laënnec and the others who had occasion to be witnesses of his work at the Charité. Laënnec said of him that "he was gifted with wondrous powers of concentration and perseverance; nothing could tire or dishearten him, indeed application seemed to be so inherent in his habits that none of his friends and fellow-workers ever saw him, through lassitude, discouragement or neglect, omit to do that which was to be done. Imbued with religious principles, he was consistent in those principles even to austerity." (Page 182, *Life of Laënnec*, Hale White, 1923.) Bayle now published a remarkable work and the first which he contributed to the subject, his *Remarques sur les Tubercules*, which appears to have entirely changed the manner of looking upon these alterations. For the first time the anatomical character

of these morbid structures is clearly given and the different parts that are occasionally affected by them set out. He finally published his *Recherches sur la Phthisie Pulmonaire* which is a mine of information and deserves the closest attention. This work, published in Paris by Gabon, Libraire, Place de l'Ecole de Médecine, in the year 1810, gives his views on the most common manifestation of tuberculosis. "C'est parce qu'on n'a pas fait assez d'attention au caractère essentiel de la phthisie pulmonaire qu'on a fréquemment méconnu les traces de cette maladie dans l'ouverture des cadavres où elle était peu avancée. Par suite de cette erreur on a été privé des lumières que l'anatomie pathologique aurait pu fournir sur les premiers temps de cette affection." This essential truth of the value to clinicians of the closest and most careful pathological work so as to clear up their conceptions of very early cases of the disease is still one of the most important factors in tuberculosis and all the more so nowadays with the X-rays to help or to hinder in early diagnosis. With this idea of the importance of pathology present in his mind he conducted a long and profound series of observations, in the course of which he finally admitted six kinds of phthisis. In reading of these varieties it is almost impossible to remember that he was making all his points without the least knowledge of the tubercle bacillus! The fact remains, however, that this knowledge was entirely hidden from him; that he was describing as "phthisis" the *wasting* which might result from any pulmonary lesion; and that it was quite natural for him to assume that "cancerous" wasting might be included with the phthisical diseases as correctly as the wasting of pulmonary phthisis. It should be noted, also, that his "*phthisie ulcèreuse*" was intended to describe the wasting noted in cases of ulcerated lungs following abscesses and not the ulceration accompanying tuberculous lesions. This is an important point, as anyone reading the title nowadays would naturally draw the conclusion that Bayle was here speaking of the latter, but it is not so. The truth is that Bayle was attempting to *divide* all cases of wasting due to lung conditions into six different *diseases* or, at least, diseases which he thought to be different. We give, then, his different kinds of phthisis with

the numbers of each kind from a total of 900 cases as laid down in his chapter IV:

Phthisies tuberculeuses	624
Phthisies granuleuses	183
Phthisies avec melanose	.72
Phthisies ulcereuses	14
Phthisies calculeuses	4
Phthisies cancéreuses	3
Total	<u>900</u>

This gives some idea of how much lung tuberculosis there was at that time in Paris; 624 cases which were marked out by Bayle himself under that category and 183 cases, described as "*granuleuse*," which we now know to have been tubercle of the lungs as well. The "*ulcereuses*" and the "*cancéreuses*" amount to seventeen cases only and these we may perhaps accept as not really tuberculous though, while some of them probably had an element of tuberculosis, the majority were either true cancer of the lungs or true cases of abscess. Laënnec had not yet divided off the cases of bronchiectasis which he was just about to do in his decisive way and some of Bayle's instances were undoubtedly of this nature; for instance, Case 39, described as "*Phthisie tuberculeuse et phthisie calculeuse*," and Case 19, a clear instance of bronchiectasis. There are, among the cases thought to be phthisis, cases of toxæmia from some source not quite sufficiently specified. But, on the whole we may accept Bayle's diagnostic clarity and take his cases as decidedly what he called them. Laënnec takes them up as described but regards the *names* as misleading unless placed, as he places them, all, except the small minority already separated, as instances of tubercle of the lungs, either acute or chronic. He was out for the "*unicité*" of tuberculosis and his remarks on Bayle's attempts to differentiate this into particular and individual diseases are dealt with, wise as they are, in the section of this work devoted to him. Nevertheless, Bayle deserves the greatest credit for his extremely good descriptions of the diseases of the chest and their pathological characters, as far as these

could be settled without the extended use of the microscope which is now thought, and indeed is, so necessary. His was "the voice of one crying in the wilderness"; the voice of one who had convinced himself that what he saw amounted to what could and would give rise to practically all the diseases which the clinician might descry in the field of chest-illness. He might, as was indeed the case, make a few errors in the exact nature of what he saw, but everything is so well described that it is quite easy to follow him. He described the appearance of the disease in its different phases but remained in ignorance of the causal identity of one phase with another. He was an exact observer but not an inspired thinker. He was incapable of what Laënnec accomplished with such apparent ease though after such inscrutable thought: "The nature of tubercles may be developed in the lungs or other organs under two principal forms—that of *insulated* bodies and *interstitial injection or infiltration*. Each of these presents several varieties, chiefly relative to the different degrees of development." If Bayle had, instead of only *observing* nature, *defined* it, he might have guessed the truth which came so naturally to Laënnec. But we must allow that, without microscopes and sections to guide us, we too, should have fallen into the same error. How Laënnec hit on the facts we are unable to imagine. There is no doubt that he was either a superlative guesser or else an observer gifted in superlative degree with the power of generalization.

But to return to the consideration of Bayle's work alone; there is no doubt that he hit on a fairly correct estimate of the number of early cases of tuberculosis of the lungs when he noted 624 out of nine hundred cases—that is 68·1 per cent—as the approximate number. We find, from the post-mortem accounts of the fatal cases cited that practically all of them had, whether with or without other other manifestations, miliary tuberculosis and this shows that he was picking his cases chiefly for this sign; the number would have been greater, no doubt, if he had been aware of Laënnec's dictum, or had accepted it, that cases of tuberculous infiltration ought to be included also, but, even so, the majority would have had this lesion as well as miliary tubercles and the totals are not far wrong. He gives

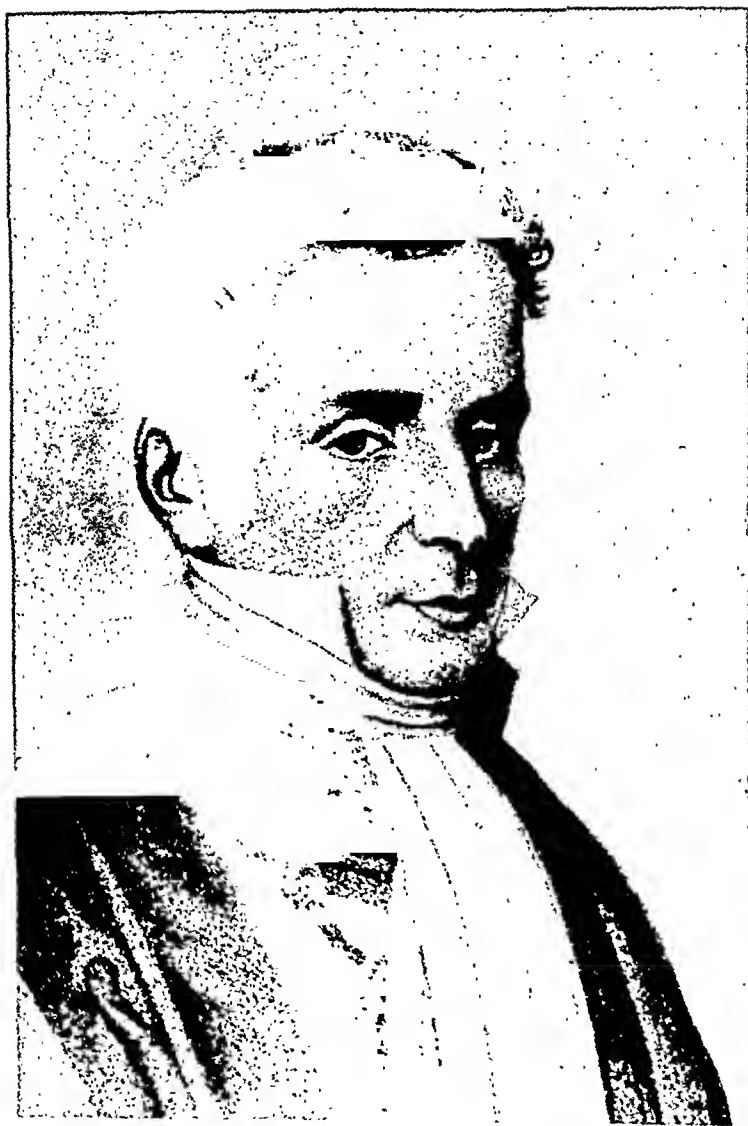
a table of the total deaths, and the times of survival in 200 fatal cases, showing again, what a large number were of the most acute variety. These were, of course, a selected group. When we add 183 *phthisies granuloses*, which he regarded as belonging to a separate disease but which Laënnec saw at once to be tuberculous, we reach to 90 per cent and with seventy-two cases of *phthisies avec melanose* the total comes to 97.6 per cent. This, allowing for cancerous cases, abscesses and bronchiectasis, is probably right as an estimate of the total number of phthisis cases in the modern sense of the term, which he saw and observed. Turning once more to the 200 cases of death which Bayle followed through their illness up to the end, we may take his second table as showing that, of these, sixty died in the first half-year or 30 per cent, sixty-four in the second half-year, or, adding them to those of the first, 124 or 62 per cent in a year. This, after all, is much the same as I found in an attempt to trace the times of survival of "acute" febrile cases in Wales, from the cards rendered by tuberculosis officers, in 1926. (Welsh National Memorial Association, *Pulmonary Tuberculosis in Wales*, A Study based on the Epidemiological Enquiry Cards filled up by Tuberculosis Physicians from 1923 to 1926.) It proves that, for a large city such as Paris, in 1810, the mortality was much like that to be estimated from acute febrile cases in the very susceptible Welsh population of Wales in 1926. It is to be noted also, that of 100 phthisics, in seventeen the larynx was diseased; and that, of 100, no less than sixty-seven presented ulceration in the intestinal canal; Calmette, however, quotes Louis to the effect that, of 215 cases, the intestine showed ulceration in 174, or 81 per cent, either a great success in finding the ulcers or a greater number than we find nowadays. I should put the number with intestinal complications at about 60 per cent, but the number varies greatly with the types of case admitted to the institution concerned and the difference need not concern us as very important. Louis noted that one quarter of all pulmonary cases had laryngeal ulceration. Ashworth Underwood, in his excellent little work on tuberculosis, agrees with the late Sir St. Clair Thomson that about 30 per cent of cases of pulmonary tuber-

culosis have some ulceration of the larynx; a decidedly greater number than was observed by Bayle who may, however, have been dealing with a less immunized type than we meet today. The numbers are not, however, very different from those that might be obtained now in any thoroughly well carried out survey and we must count them as very creditable to Bayle's methods. He divided tubercle of the lungs into four types according to the period of survival, but these distinctions do not count for much and are here omitted. It is probable that he infected himself in the course of his numerous post-mortems of tuberculous subjects; it seems to me that both he and Laënnec owed their deaths from tuberculosis to this source. Laënnec's mother, as a matter of fact, died from this cause and is, I think, certain to have infected her child; but the re-infection that finally carried off both these observers was probably that of the post-mortem room. Bayle's was a consistently valuable life and his death was a great loss to the study of tuberculosis. He died on the 11th day of May, 1816, after having brought the study of his subject to a newer and a more intelligible stage.

RENÉ THÉOPHILE HYACINTHE LAËNNEC (1781-1826)

RENÉ THÉOPHILE HYACINTHE LAËNNEC was born on the 17th of February, 1781, at a house on the Rue du Quai at Quimper, in Brittany. He is said to have been descended from Malherbe, the Breton poet, and there are many more or less credible stories about his family which I do not propose to recount here. Suffice it to say that he came of a race which was a very ancient one and that many had been lawyers and magistrates in the neighbourhood for a number of generations. His father, Théophile-Marie, was born in 1747, became a member of the Bar and held various appointments, such as *avocat au parlement*

de Bretagne, etc., and, though possessing a certain amount of genius, had also a frivolous and light side to his character which was to have an unfortunate effect on his relations with his son later on. As Saintignon says about Laënnec: "de son père, à la vérité, il ne tiendra bien peu; l'homme grave et un peu triste qu'il sera tout sa vie, ne ressemble pas à l'homme léger, souriant, peut-être trop frivole, que fut son père." He married a young lady, by name Michelle Alexandre Félicité Guesdon, daughter of the *sénéchal* of Regaires. She died when Théophile was only six years old, at the confinement of her fourth child, who died at the same time. There is a story (Lallour) that she died of phthisis, and this may very well be true as there is a tendency for this disease to be activated by childbirth, but on the other hand she may have died from one of the other perils that surround the mother at such times. According to Saintignon: "she was probably far from strong, and it is possible that she was tuberculous. It is evident that Laënnec owed his feeble constitution and his tuberculous 'terrain' to his mother, a tendency which, all too soon for the progress of science, proved fatal to him in his turn." The remaining three were René Théophile, Michael or Michaud, and a little girl, Marie-Anne. All three were sent from home to their uncle, the Rector of Elliant, their father not being inclined to keep them. There they remained for only a short time as their uncle was transferred to be Canon and Vicar-General of the diocese of Tréguier where he was unable to keep the children. Soon after his promotion he was obliged to fly from France owing to the general proscription of the clergy and settled in England where he, too, died of phthisis, marking out a bad family history for René. The two sons first returned to their father, but a little later, were sent to another uncle, Guillaume François Laënnec, a doctor, at Nantes. Guillaume Laënnec, married to Mademoiselle de Gennes de Matignon, was a good friend to René Théophile, as were also his wife and her mother, both devoted to their nephew though there were three children already. Later, Michaud returned to his father, at the latter's request, and René Théophile might have gone back also but preferred to stay on the excuse that he wished to become a



RENE THEOPHILE HYACINTHE LAËNNEC (1781-1826)

medical student at Nantes, his uncle being himself a doctor and also Rector of the University. Théophile, then, became practically one of the family at the Place de Buffai where his uncle Guillaume was, in every respect, a father to him. The times were very bad, however, and the revolution was in full blast. We are told that no less than 3000 persons were put to death in Nantes alone and that, when execution by means of the guillotine was found too slow, they were actually cut down in ranks by chain shot. "It was common for men and women to be tied together naked and then thrown into the river. A permanent guillotine was put up in front of Dr. Laënnec's house. Many heads fell from it; the sight was so terrible that the children had to go to school by a back way" so as to avoid it. All the same their education does not seem to have been much curtailed by "the fiendish times in which they lived." Théophile, wearing a sword and aged about thirteen, was constrained to take part, in June, 1793, in the *fête de l'être suprême* which had been instituted by Robespierre's order. His father having married again was anxious to persuade Théophile to leave his uncle and come to him but the boy decided to remain at Nantes for the reason already given. He was entered as a student at the School of Medicine at Nantes in September, 1795, being then only fourteen and a half years old. He was very short of money owing to his father's omission or inability to provide for him, but his uncle seems to have come to his rescue when necessary. He was appointed Military Surgeon of the Third Class in the same year—the army must have been rather hard up for medical officers!—and was probably attached to the Hôpital de la Paix with his uncle, who had been taken into the army also. In 1797, both went to the *fraternité*. At about this time, being devoted to music, he learnt to play the flute well, an accomplishment which often came to his aid in future years. On January 22, 1800, he was appointed an *officier de santé* and was sent with an expedition to quell the insurrection in the Morbihan district between Nantes and Quimper. His father, who had shown considerable reluctance to afford the necessary help to Théophile, was at last, in 1801, bound down by a legal document to allow sufficient money for his board, lodging and student's fees in

Paris, and, thus provided, he left Nantes for the capital. At first he went to his brother's lodgings at 947, Rue St. Dominique d'Enfer. This proved too small and they removed, later, to Place St. Michael, 514. Théophile entered at the Ecole de Médecine on May 2, 1801, and settled down to work. "There were then two schools of thought in the medical world of Paris; that of Pinel who taught at the Saltpetrière," says Hale White, "its disciples being rather theoretical and interested chiefly in the classification of disease," and that of Corvisart at the Charité. Laënnec's previous training at Nantes and the example of Bichat's work on morbid anatomy inclined him to the second; some say that Bayle, his senior by seven years, urged him the same way. Here Bayle and Laënnec met and became friends, a friendship which was destined to last for life. Although there is evidence that he did not like him, Théophile still attended the teaching of Corvisart with marked attention. There is preserved at Nantes a note-book entitled *Aphorisms recueillis aux leçons du Citoyen Corvisart par René Théophile Laënnec* containing 135 articles. Chosen a member of the Société d'Instruction Médicale in May, 1801, he was admitted to l'Ecole Pratique during the same year. "Here he found Dupuytren working hard at morbid anatomy; Bayle was helping him and Laënnec enthusiastically joined them" (Hale White). He appears to have been one of the two students placed, under Hallé, in charge of some patients being tested with a new febrifuge devised by Seguin, a chemist,—a solution of gelatine as one gathers—to observe the results over some months. Though it excited a great deal of attention and curiosity the remedy seems to have given no results and to have been reported as without any special value. Hallé, however, took pains to compliment his two student observers on their work when he made his final report. Laënnec's first publication was in the *Journal de Médecine, de Chirurgie et de Pharmacie* of June-July, 1802, on a case of mitral disease. A much more important paper was to follow in the same journal, two months later, the "Histoire d'Inflammation du Péritoine," which aroused considerable discussion and was thought greatly to the credit of the author. He also made public a letter to Dupuytren on

the "tuniques qui enveloppent certains viscères." In 1805, however, he published a *Note on Pathological Anatomy* "which led to a sharp controversy with Dupuytren as to which should have priority for the classification therein mentioned" and was the occasion for a quarrel which lasted for some time. In 1804 he became one of the editors of the *Journal de Médecine, de Chirurgie et de Pharmacie*, a paper which seems to have been Corvisart's own particular means of communicating his work to the medical public, and thus, as the result of his many and frequent contributions, attained a position of considerable importance. In 1804, he read, before the *société*, his "Memoire sur les Vers Vesiculaires" which appeared (B.S. No. 10, Juin-Juillet, 1805) in the *Bibliothèque Médicale* (IX, p. 266), and marked him out as a coming man. In his treatise on the doctrine of Hippocrates he dismissed Pinel's six varieties of fever and reverted to those of Hippocrates, two only, "intermittent" and "continuous." It is as well to consider, in this place, Laënnec's health which, at about this time, gave cause for some anxiety. His bad history has already been mentioned. He had probably spent the first six months of his life with a tuberculous mother, a period particularly susceptible, and he had then passed some time with his uncle, the priest, before the latter left his parish to become Canon of Tréguier, which must have been another period of "contact," as this gentleman died shortly afterwards of phthisis. Laënnec appears to have lived an unhygienic life in Paris. He had, for about three years, bad attacks of dyspnoea, these being better when he was able to take long walks and much exercise but very bad in the course of his usual sedentary life. Rouxau tells us that he often sat naked to the waist at his writing which indicates a considerable degree of sweating. But though, at times, filled with a suspicion that he might be tuberculous he remained, on the whole, an optimist as to his health at that time.

Laënnec, then, must have been at least infected at a very early age; it is not possible for an infant or child to be in contact with infected adults without getting enough of the virus to make him infected in his turn. But such primary infections either produce tuberculosis early or lead on to a condition in which

most of the small lesions heal up into scars which, even though they may still contain living tubercle bacilli, are relatively static and prone to be quiescent. Laënnec's energetic life, spent in hard study or in 'occasional shooting trips and long walks, makes one think that he had attained to relatively good health in spite of his early "contacts." It seems to me likely that it was to the re-infections of the post-mortem room, frequently repeated in the course of many autopsies carried out without any attempt at sterilization, that he contracted the re-infection which led to his early death. The same applied to Bayle and, probably, also to Bichat. We know that Laënnec infected his fingers at least eight times during the course of his post-mortem examinations and we must assume that he contracted respiratory and intestinal infections as well. On the whole I feel inclined to think that he, and also Bayle, must have owed their early deaths to post-mortem infections—but, of course, there remains the possibility of a tuberculosis developing from childhood. Of one thing at least we may be certain; that at the very time when he was engaged with Bayle and Dupuytren in constant anatomo-pathological work, his health was already suffering and that he was getting frequent attacks of "asthma" and such-like symptoms. His poetic rendering of his state, or rather his poetic attempt to describe what he already felt in general terms, may be seen plainly in the following verse quoted by Rousseau:

" Santé, trésor qu'on commence à connaître
Lorsque déjà, loin de nous il s'enfuit ! "

In 1803 he had such a want of energy and such an inability to work that he felt at the end of his forces and longed to go away to Nantes, but his father never would supply him with the necessary money. His father, in fact, seems to have put away, under rather disgraceful circumstances, about 50,000 pounds, which had been, from time to time, committed to his charge from the *décimes du clergé* which he usually collected, and poor Théophile was compelled, in the words of Rousseau, "to try to avoid this coming to light" and to arrange the interest, etc., on the money with Paris business men instead of getting a holiday.

After the winter of 1804-5 he took part in a long shooting expedition, his first since he had been in Paris, walking for twelve hours over more or less cultivated country. He felt very much better for it the next day. His asthma had considerably diminished. In view of this experience he determined to take a holiday from time to time as it might be possible. "In examining some tuberculous vertebrae," says Rouxeau, "he gave himself, by accident, a cut with a scalpel on the index finger of the left hand. Following this, there was a tuberculous lesion of an undoubted nature which, however, quickly healed." This rapid healing shows, I think, that he had already met the tubercle bacillus and that he was then in a state of relative immunity against such attacks. It appears that, as above stated, he had pricked himself with tuberculous material at least eight times, especially in 1804.

In the autumn of 1807 his father made his appearance in Paris and insisted on staying with Théophile, whom he plagued for aid in seeking employment and abused for not giving him more money. This visit seems to have been a terrible nuisance to Laënnec, but he had, of course, to do his best to shoulder the burden. After a bad attack of "cholera morbus," however, which came on after much indigestion and bad intervals of hypochondria, he determined that he must get rid of this incubus and frankly told his father that he was obliged to go to Courville for his health and moreover, *went* there and had a fortnight's shooting in the neighbourhood of Soissons, after which he was very much better and also, indeed, rid of his parent. From 1808 to 1811 he suffered from considerable internal trouble and was, moreover, prevented by an illness of Bayle, his great friend, from leaving Paris. In 1812 and 1813, too, he suffered a good deal from his hypochondria but worked constantly, finding much relief from going to bed early and taking every opportunity of walking. He lost his aunt, Desirée, on August 13, 1813; a loss which he felt a great deal. He would have gone away and had some exercise and perhaps some shooting at Soissons, but the continued illness of Bayle and of another friend kept him in Paris. In the following September, however, he managed to escape for a fortnight and, in spite of a slight

attack of gout, to get some shooting which made him much better for the time being. The Pomperys, his cousins, asked him to stay with them and he did so to the great benefit of his health but, during 1814, he suffered again from terrible fatigue after hard work and appears to have been in a thoroughly bad state. Frightened by this, he started on three month's holiday in Brittany during the following summer and, again, was much better for it. But his bad health continued to plague him. Rouxeau says that "in 1816, after a bad attack of asthma, he developed a severe and continued headache; so bad that, at the time of a paroxysm, his head felt as if it were in a vice; he could not, at such times, do any intellectual work." It was in 1816, too, that he was nominated to be Medical Officer to the Hôpital Necker after much difficulty and much competition. As he says himself, in a letter, "Nonobstant ce que tu as lu dans le Moniteur, je serai sous huit jours, médecin de l'Hôpital Necker." The practitioners attending for instruction, had, until then, only had, as a help in diagnosis, the method of percussion, made popular by Corvisart. They were now to have experience of the discovery of Laënnec!

Théophile's appointment to the Necker Hospital marks a stage in his career. He was now thirty-two years old and he had succeeded in entering this establishment at the cost of considerable trouble, it is true, but successfully and finally. What we are most impressed by at this time is his extraordinary precocity. In the twelve years that had elapsed from his joining the classes in Paris until this appointment, the years of his student life are to be included, and only nine as a qualified doctor, and yet we see him standing forth as the author of several remarkable papers, the friend of Bayle and of Dupuytren, the pupil of Corvisart and of Bichat, the editor of one of the most advanced medical journals, a marvellous authority on morbid anatomy and a clinician who was to teach a new method of chest examination, not only to his contemporaries but to many future generations of physicians including our own. Laënnec, at the time of his entry into the Necker, although he practised "immediate" auscultation, still hated the method on grounds of the frequent reception of ectoparasites which it entailed, a much commoner

occurrence then than it is now, also on grounds of what was then regarded as decency. His mind, perhaps, was working on how to avoid it when, one day, he happened to walk through the Louvre, his observant eye taking notice of everything he saw. I put down his remarkable experience in the French of Rousseau as it is a good example of the prepared mind seizing upon something that might have meant so little to the average doctor and yet meant so much to him.

“ Dans un coin, une joyeuse marmaille est fort affairée aux prises avec un amusement qui absorbe toutes ses facultés d'attention. Il avait là deux groupes de gamins postés aux deux extrémités d'une longue poutre; les uns grattent ou frappent légèrement le bois avec une épingle; les autres, à tour de rôle, collent leur oreille contre la poutre et ce sont les cris d'admiration en entendant de grands fracas les chocs si légers que l'expéditeur a lui même quelque peine à discerner. . . . Trait de lumière pour notre promeneur qui s'est arrêté un instant à considérer le tableau.”

Now there happened to be at the Necker a young lady patient whom he had just been visiting and whose heart was clearly affected. He was anxious to examine it and yet the lady objected to a human head being placed immediately on her breast, as, indeed, the doctor himself did also. The game which he had just witnessed gave him an idea. Why not try something like the poutre on a small scale for examining this virtuous patient without inflicting any shock upon her sensitive feelings? Arrived at the hospital the next morning, he took a visit-card, rolled it up tightly and tied it firmly. “ He would have wished to eliminate the central canal altogether but he could not make it sufficiently tight for this and so had to leave it open to some extent; and this was a happy chance because he owed to his omission the discovery of pectoriloquy that, with a fully closed cylinder, he could not have discovered and yet which constituted perhaps the most important part of his experiment.” Placing this improvised instrument upon the lady's heart area he was astonished to find that the sounds came to his ear much more clearly than he had ever heard

them before! The origin of the stethoscope was in his hand. "Mediate auscultation" had been made known to him! It only required the intervention of his genius to produce the instrument with which we are all familiar today. He started at once and tried his roll of paper on the numerous chest cases that were gathered under his charge at the Necker. In all of them he found the instrument to give him valuable hints. But there was one in whom he found something that he had never even heard before. This was a lady patient, Mademoiselle Grigy, in whose chest he could hear the most remarkable approach of the voice up the tube, as it were, over a very restricted area, when she happened to say something while he was examining her. His mind worked furiously. Could it be true? Was he hearing a new sound? He tried his instrument on the chests of all the patients suffering from phthisis under his care. Yes! it was there in several of them. The very patients whom he had suspected of having cavities! He had only to wait until the next post-mortem on one of them to know finally that he had hit upon a sure guide to the presence of a vomica, the sound which he called "pectoriloquy." He verified it upon the normal chest by hearing it typically over a natural cavity, the trachea. The sound was there. The principle was right. The instrument was a success. A sample of it, with the words "Donné par Laënnec à Recamier," used to be kept in the latter's study. It may now be in the Laënnec Museum. But it was not sufficiently perfect to satisfy its inventor. He tried other models, some of paper and some of wood. For the latter he learnt to "turn," with the proper instrument. Finally he satisfied himself that the best material was wood and produced a stethoscope of considerable length but still suitable; a stethoscope that could either be used for the heart with nothing but a hole through it, which he found was convenient but not really necessary, and a portion with a large hollow cone leading into the cavity of the instrument which could be used for the chest and for the ascertainment of the presence or absence of "pectoriloquy." The hole had to be small. "Avec un canal plus large les resultats furent moins bon."

We all know the many varieties in use today; the single, the

Fig 1^{re}



Fig. 2.

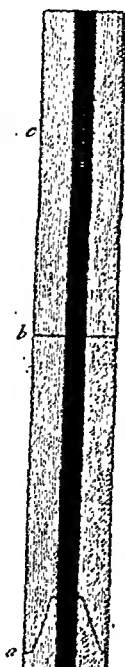


Fig. 3.

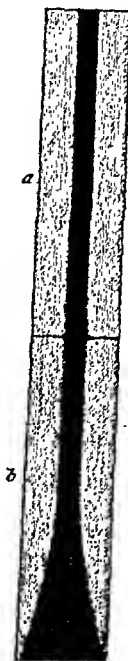


Fig. 4:



Fig. 5.

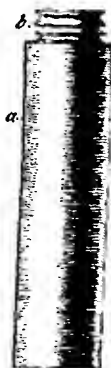
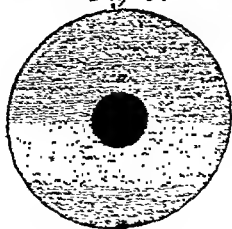


Fig. 6.



LAËNNEC'S DRAWING OF HIS FIRST STETHOSCOPE

double, the tubes of rubber and so on, but we must acknowledge that, for the mere hearing of the sounds of respiration and of pectoriloquy, apart from the convenience of the operator, the wooden single stethoscope of Laënnec is as good as any.

On the 28th of February, 1818, he read to the Académie des Sciences a memoir "sur l'auscultation à l'aide de divers instruments d'acoustique employés comme moyen d'exploration dans les maladies des viscères thoraciques et particulièrement dans la phthisie pulmonaire."

On May 1, he commenced his communication to the Faculté de Médecine, reading a memoir as above, dividing the results of the method into four portions; Voice, Respiration, *Râle* and the Circulation, and Heart. This and the subsequent publication of his great book, *A Treatise on the Diseases of the Chest and on Mediate Auscultation*, completed on the 16th of August, 1819, and translated into English by John Forbes in 1821, "printed for Thomas and George Underwood, Fleet Street, in 1821," represent, to my mind, the great fruition of the work of Laënnec. He was destined to achieve, even in his short though laborious life, a great notoriety and a wonderful reputation but, for sheer performance, we must acknowledge that he had well served his time when he brought out this superb and magnificently written volume.

Let us return a little and consider the attitude to tuberculosis current at the time.

As Rousseau says: "One lived as well as one could on the little that was known of the work of William Stark, unhappily interrupted too soon by the premature death of this young savant." As a matter of fact William Stark had written his description of tubercles and their breaking down into vomicae, the relative loss of vascularity around them by the sealing off of the vessels and the opening into them of bronchi and bronchioles which had likewise been interrupted in their course by the tuberculous process, nearly twenty years before the birth of Laënnec; but had written of them briefly though clearly and his work had remained unpublished until 1782. One imagines that this work was only slightly known and appreciated by the profession in France as, indeed, it was equally unread in

the England of that time. But it was not altogether ignored by the learned there and it probably served as the inspiration of men like Bayle and Laënnec. Bayle had already published his 900 cases of phthisis due to respiratory disorders and had declared at least 624 of them to be tuberculous in origin. Laënnec had gone further, had recognized the tuberculous nature of the *phthisie grānuleuse* which Bayle had stated to be a separate development, had recorded his belief that the 183 cases of this kind should be added to the total of tuberculosis described by Bayle, and had thus put the crowning touch to his studies of this disease by announcing the *unity* of the various manifestations of tuberculosis under this one heading. It was now his to discover a means by which all these tuberculous signs as well as many others, non-tuberculous, affecting the lungs and the heart, might be recognized during the life of the patient.

It is as well, I think, to quote Laënnec's own words on his discovery as translated by John Forbes:

“In 1816, I was consulted by a young woman labouring under the general symptoms of diseased heart and in whose case percussion and the application of the hand were of little avail on account of the great degree of fatness. The other method just mentioned (immediate auscultation) being rendered inadmissible by the age and sex of the patient, I happened to recollect a simple and well known fact in acoustics and fancied it might be turned to some use on the present occasion. The fact I allude to is the great distinctness with which we hear the scratch of a pin at one end of a piece of wood on applying our ear to the other. Immediately, on this suggestion, I rolled a quire of paper into a kind of cylinder and applied one end of it to the region of the heart and the other to my ear and was not a little surprised and pleased to find that I could thereby perceive the action of the heart in a manner much more clear and distinct than I had ever been able to do by the immediate application of the ear. From that moment I imagined that the circumstance might furnish means for enabling us to ascertain the character not only of the action of the heart but of every species of sound produced by the motion of all thoracic viscera.”

Laënnec began as we have written above to find types of cylinder or "stethoscope," as he called his tube, more fitting than his roll of paper. "Bodies of a moderate density such as paper, the lighter kinds of wood or Indian cane, are those which I have always found preferable to others." But he pauses. "The employment of this new method must not make us forget that of Auenbrugger. On the contrary the latter acquires quite a fresh degree of value through the simultaneous employment of the former and becomes applicable in many cases wherein its solitary employment is either useless or hurtful." There we have it at last: *the linking up of two wonderful discoveries of two men of genius*; Auenbrugger and Laënnec! I wish to say that these two methods, linked with the far older one of palpation, formed practically the only approach to the study of internal diseases of the chest for at least 100 years, from, say, 1820 to 1920. Nor have they yet been abandoned or their utility come to an end. It is all very well for the physician attached to a well provided hospital, or his colleague in close enough touch with him, to enjoy the advantages of the X-ray, but what of the thousands of doctors out of touch with these facilities? I think we may say that the great majority of doctors still rely on the combined methods of percussion and auscultation for the examination of the chest cases that they see in their everyday work. Laënnec lost no time in gathering the honey from his great collection of flowers, the numbers of cases of all kinds that were at his disposal. He may be quoted again, though most of my readers are probably well acquainted with the passage in connection with the auscultation of the voice. It seems worth quoting although we have already given the facts.

"In the very earliest period of my researches on mediate auscultation I attempted to ascertain the differences which the sound of the voice within the chest might occasion. In examining several subjects with this in view I was struck with the discovery of a very singular phenomenon. In the case of a woman affected with a slight bilious fever and a recent cough having the character of a pulmonary catarrh, on applying the cylinder below the middle of the right clavicle while she was speaking, her voice seemed to come directly

from the chest and to reach the ear through the central canal of the instrument. This peculiar phenomenon was confined to a space about an inch square and was not discoverable in any other part of the chest. Being ignorant of the cause of this singularity I examined, with a view to its elucidation, the greater number of patients in the hospital and I found it in about twenty. . . . The subsequent death in the hospital of the greater number of individuals who had exhibited this phenomenon enabled me to ascertain the correctness of my supposition; in every case I found excavations in the lungs of various sizes, the consequences of the dissolution of tubercles, and all communicating with the bronchia by openings of different diameters. . . . This circumstance naturally led me to think that *pectoriloquism* is occasioned by the superior vibration produced by the voice in parts having a more solid and wider extent of surface, and I imagined that, if this were so, the same effect ought to result from the application of the cylinder to the larynx and trachea of a person in health. My conjecture proved correct."

I cannot refrain from quoting this remarkable example of Laënnec's research methods but I shall say no more about them here as my readers already know all that is to be known about mediate auscultation. Suffice it that he laid down a system of stethoscopy and described, in an enduring way, the various types of sound to be heard over the normal and the abnormal chest; vesicular breathing, bronchial breathing, diminished breathing; the modifications of the voice as, for instance, those to which he gave the name "*pectoriloquy*"; and the sounds on the production of fluid alone, air and fluid, etc., such as *aegophony*; and all this with a careful application of percussion to supplement the stethoscope. It is a wonderful record; and, taken with his morbid anatomy findings, the demonstration of the "*unity*" of tuberculous lesions, the discovery of "*dilatation of the bronchia*," "*bronchiectasis*" as we now call it, the profound study of *emphysema* of the lungs, and the numerous non-pulmonary observations on *peritonitis*, brain tumours, worms, etc., it constitutes an almost

incredible amount of original work crowded into one short life. For Laënnec was to die young. It seems possible, as we have said, that both he and Bayle contracted the tuberculosis that killed them as a re-infection gathered in both, during their long and arduous observations at the post-mortem table; nothing was then known of the infectious nature of tuberculosis or of the necessity for sterilization of hands, instruments, etc.; and yet Laënnec speaks of having carried out at least 200 autopsies on tuberculous cases alone, while Bayle devoted his short life to pathological anatomy and tuberculosis.

It is necessary for me to remember that this is not a life, but the summary of a life of Laënnec; it is so tempting to deal with him as if it were. But I must hurry on. His heart was pining for his old place, Kerlouarnec, where the sea air had so often brought him back to health; where there was shooting and sport to be had; where there was peace from the jealousies and misunderstandings of his professional brethren. And he himself had spent many holidays there from time to time; once for a period of broken health of nearly two years. He became Professor of Medicine and held clinics at the Charité from 1823. In 1824 he was married, not in a spirit of romance but rather, from his point of view, for the sake of quiet and a home and, for hers, freedom from the scandal that might arise if she were merely to live with him and take care of him. She was his relative, Madame Argou. The marriage proved a great success. He died on August 13th, 1826, at Kerlouarnec, the "Place of Foxes"; the beloved place where he had passed the happiest days of his life.

What, then, of the percussion of Auenbrugger and the mediate auscultation of Laënnec? I place them together as a single technique for the examination of physical signs in the chest and I maintain that they represent the greatest physical discovery yet made for the investigation of intra-thoracic disease. They have not, of course, any bearing upon the wonderful and new discovery, made solely by the X-ray, that there is a *pre-clinical* stage at which tuberculosis may be found and treated with a degree of success impossible when it has run on to the stage of clinical diagnosis. But it is still the case that the majority of those

doomed to get tuberculosis only discover the fact when signs and symptoms have become positive. To such I say that, though the X-ray picture is now a necessity as well as a mere physical examination, percussion and auscultation are still processes by which the state of the lesions can be diagnosed and amplified and, subject to a positive sputum examination or other means of certainty, made more exact than a mere statement that the lesion, whether acute or chronic, exists.

It is impossible seriously to abridge Laënnec's work which must be either read thoroughly in the original or taken for granted as is done by a majority of physicians nowadays. Nevertheless, it seems worth while to attempt to set forth his *methods* in two at least of his descriptions; tuberculosis and bronchiectasis. Let us take Forbe's translation and quote Laënnec in his account of how tubercles develop. "The matter of tubercles may be developed in the lungs or other organs under two principal forms—that of *insulated bodies* and *interstitial injection or infiltration*." It was thus that he announced the *unity* of tuberculous disease under one heading. Other observers, including the great Bayle himself, had failed to appreciate that these two manifestations were really of the same significance. Laënnec's observation in this summing-up is of essential importance. We must remember that he had nothing but his own intense understanding to guide him. Examination of material by section and the microscope was, perhaps, at his disposal but only to a slight extent, and the convincing proof, the finding of tubercle bacilli in both, was entirely unknown. "Whatever be the form under which the tuberculous matter is developed, it presents at first the appearance of a grey semi-transparent substance which gradually becomes yellow, opaque and very dense. Afterwards it softens and gradually acquires a fluidity nearly equal to that of pus; it being then expelled through the bronchia, cavities are left, vulgarly known by the name of *ulcers of the lungs*, but which I shall designate *tuberculous excavations*." Such was his description of the changes which tuberculous matter undergoes in consistency from the "grey semi-transparent substance" onward.

We shall quote only one of his post-mortem examinations, just to show his methods.

“ A man, aged thirty-two, affected during the preceding six months, at intervals, with mania, was brought to the Necker Hospital, 26th December, 1817, in a state of stupor, and died a few days afterwards. Sufficient cause of death was found in the brain. [This is often the case with persons suffering from mental disease. They seem to get tuberculosis without feeling it much, and often without its being noticed except at post-mortem.—S. L. C.] I shall only notice the condition of the lungs. The left lung was one-fourth less than the right and adhered by numerous cellular attachments to the pleura. It was, throughout, sound and crepitous, but contained about seven or eight tubercles of the size of hemp-seed, having a yellow and opaque speck in the centre. The right lung was in its summit attached to the pleura by old adhesions, and contained, in this place, an excavation capable of holding an egg. This cavity, which was filled with a clot of blood, was lined by a semi-cartilaginous membrane, a quarter of a line thick, of a pearly-grey colour and very smooth and polished, yet having little tuberosities on its surface. Several bronchial tubes of different diameters opened into it. The rest of the lung was perfectly crepitous throughout, even around the excavation, but contained an immense quantity of tuberculous granulations, of the size of a millet seed at most, besides three or four other tubercles of a larger size and already yellow, opaque, and somewhat friable towards the centre.”

The picture is quite ordinary except that it is painted for us so long ago. The cavity at the summit of the right lung, probably the result of an old secondary infection; the large miliary tubercles in the left lung and “ three or four other tubercles of a larger size ” in the right; these being the result of a slight miliary spread some time before; finally, the immense number of tuberculous granulations, the most recent miliary result of the eating into the pulmonary artery of the right side at some point invaded by a tuberculous lesion; the whole event is quite

obvious now—but must have presented points of obscurity only to be enlightened by a man of genius then.

Let us now turn to bronchiectasis of which Laënnec was actually the discoverer, other observers who had noticed the dilations having attributed them to phthisis. I shall only describe one autopsy but it will be enough to carry conviction:

“ Acute dilatation of the bronchia after whooping cough. . . .

A child three and a half years old, and affected with whooping cough for three months. . . . Dissection thirty-six hours after death. . . . The left lung was sound in the upper parts, but the inferior lobe was hard, livid, and slightly adherent to the costal pleura. On cutting into it an ounce and a half of fetid pus, exactly like what had been expectorated, made its escape from a multitude of round, smooth cavities, varying in size from that of a large pea to that of a finger-end. On further examination it was found that these cavities were connected with, and were in fact mere dilatations of the bronchia. Each bronchial branch, after running about half an inch into the lung, became gradually enlarged and finally terminated in a cul-de-sac, constituting one of the cavities above mentioned. Towards their termination, most of these dilated tubes would have admitted a little finger; and the smaller ones would have admitted an ordinary quill. In their course they gave off branches which, after running, at most, two inches, terminated in similar culs-de-sac. The mucous membrane lining these tubes was throughout of a deep livid red. These dilated tubes were so numerous that an incision could not be made without dividing many of them: they constituted at least three-fourths of the volume of this part of the lung. The intermediate substance was of a greyish colour, compact but flabby, and retained no trace of its natural cellular structure. The right lung was sound.”

So it was with other pulmonary diseases which he described; emphysema, pulmonary apoplexy, (infarct of the lungs as we should call it), lung oedema and pneumonia; in all he gave accurate accounts of what took place and very practical advice as to treatment. He was a real chest physician as well as a

great authority on all diseases. Remarkable successes were to come to him; many of them very shortly before his death, which took place at Kerlouarnec, the "Place of Foxes," where he had spent so many pleasant holidays and where, far from professional jealousies, insincerities, and the probing vanities that were bound to enter into such a life as his in Paris, he could die in peace. In his great book, he signed himself: "Regius Professor of Medicine in the College of France, Clinical Professor to the Faculty of Medicine of Paris, Physician to Her Royal Highness the Duchess of Berri, etc., etc., etc." We bid him farewell as a genius in invention, a close and accurate observer of his patients, both during their lives and after their deaths, and a man, withal, gifted with a profound and lasting faith; faith in the value of science, faith in the close application of the human mind to human needs, faith in his own star!

PIERRE ADOLPHE PIORRY (1794-1879)

PIORRY was Professor of Clinical Medicine to the Faculty of Medicine of Paris, Doctor to the "Charity," Member of the Academy of Medicine and several other bodies of distinction, and with a practice which became more and more extensive as he became widely known for his work on *percussion médiate*. He was born at Poitiers on a date as to which there is a certain amount of doubt, but which is stated by Maurice Genty to have been December 31st, 1794. He commenced his medical studies at the age of sixteen but was obliged to break them off as a result of conscription and started out as a surgeon to the Army of Spain, where he served during 1813 and 1814. He spent fifteen months at Barcelona at the Hospital of Atarazanas where he studied yellow fever, hospital gangrene and syphilis and also the military surgery of wounds. He returned to France after the campaign and, in his *Exposé de Titres pour la Chaire*

vacante à la Faculté de Médecine de Paris, gave as his place of education the Ecole Pratique of the clinic of the "Faculté et les Hôpitaux." He stated, too, that he had been a pupil to Pinet, Corvisart, Boyer and Roux. He was received as a doctor in 1816. He was always a great writer and one idea of his was: "Pour s'instruire il est bon de donner des leçons aux autres"; a sentiment the truth of which all teachers will acknowledge. He was charged with the Hospice de la Salpêtrière and afterwards became the Professor of Clinical Medicine at the Pitié. It was in 1827 that he made his great discovery of *percussion médiate*. This is how Maurice Genty describes the matter :

"A slight attack of pruritis of the skin of the chest led to his scratching the sore place through the shirt that covered him, during which he thought that he detected a sound. He interposed a piece of silver, scratched over it and obtained a still louder sound which varied according to the density and the elasticity of the organs situated underneath. Next day, at the Pitié, he started to try his method with proper controls and was led to the establishment of the rules for 'percussion médiate'."

It seems that Piorry was a very religious man and that, after a visit by Laënnec and an opportunity of seeing the latter demonstrate his method of *auscultation médiate*, he resolved to imitate him in the discovery of some really great clinical advance if prayer could do it. "Pénétré de l'idée de Dieu et confiant en sa bonté infinie il lui adressait du fond du cœur de ferventes prières pour qu'une découverte analogue à l'auscultation et susceptible d'être soumise à des recherches expérimentales vint à se présenter à son observation." Three months later he had the happy fortune to make his great find. To Piorry, at any rate, the sequence of events was clear; the opportunity had been sent to him from above. At any rate there is no doubt that he added greatly to the value of Auenbrugger's percussion and brought it into the easy reach of the ordinary medical man. In his *Souvenirs anecdotiques et médicaux* (1927), Guéniot describes him at work. "A new mode of exploration! Seated on a high stool beside the bed,

Piorry asked the sick person to approach him. Then, placing his pleximeter on the regions about to be explored, he sketched on the skin, with a dermatographic pencil, the various particulars that percussion revealed. Finally, the bust of the patient, covered with blue or red lines, presented the appearance of a geographical map; and then, the stool having been transferred to another bed, the demonstrations continued." For his discovery of mediate percussion Piorry was awarded the Prix Monthyon by the Académie des Sciences in 1828. He had large numbers of medical men both from France itself and from foreign countries to observe his new method. Amongst them were many Englishmen and Americans and it is to one of these, his name now forgotten, that we owe the modification of the pleximeter method by the use of the finger of the left hand as a pleximeter to be struck with the finger of the right; the new method now almost universally used and for which a great deal of convincing argument might, perhaps, be advanced. Piorry, however, greatly preferred his own pleximeter of ivory and maintained his position for the following reasons, given in his work, *Du Procédé Opératoire à suivre dans l'Exploration des Organes par la Percussion Médiante*, published in Paris and in London in 1831:

The numbers are taken from Piorry's book and are in a series describing the pleximeter and its uses:

"17. A question of importance occurs here. Should one employ mediate percussion without the pleximeter, and can the fingers themselves replace it? Let us compare the two methods of exploration.

"18. The pleximeter is much the best because the different points that make up its structure are formed of one homogeneous substance. The finger, be it said, is far from having its substance made up of a composite and identical material.

"19. The more solid the pleximeter the purer are the sounds and the more definitely connected with the state of the subjacent organs. On the other hand, the finger, both on the side on which one percusses and on the side on which it touches the sick person, is covered with soft tissues.

"20. The pleximeter gives a louder sound and the

sensations that it communicates to the striking finger are more marked in proportion as it is so much the thinner; whereas the diameter of the finger on which one has to percuss varies from six to twelve lines in thickness. The instrument ought to be of the same thickness throughout whereas the finger, thick towards the metacarpal side, gets proportionally thinner as it grows nearer to its extremity.

"21. The plainer the surface presented for percussion the less it exposes the operator to the difficulty of striking perpendicularly; but the finger is convex and if one does not strike precisely on the middle of the phalanx one receives nothing but deceiving impressions."

And so on with arguments which sound very convincing but which are, according to my thinking, rather equivocal, though brought forth in the most sincere manner possible, Piorry goes on to describe the various conditions in the chest and in other parts which can be defined by means of pleximetry and his book is well worth reading but does not need much notice here because pleximetry is now so universally practised that everyone is familiar with it. Suffice it so say that he made it the popular measure that it is and that the profession owes a very great debt to the inventor of the method. It is true, nevertheless, that the fundamental technique of percussion is Auenbrugger's contribution to medicine and that, much as we owe to Piorry, the real discovery of percussion, apart from any means of performing it, is Auenbrugger's alone.

Piorry was a great writer and a great controversialist. He made his presence felt in the teeming medical life of Paris as an original and sincere man of strange but decided opinions and wide experience of medical problems. He lived to the great age of eighty-four years as an exceedingly well-preserved individual, sound in body and mind. Let us picture him as a great doctor, a poet—he wrote much poetry—and a wonderful creature in every way; but above all as the introducer of a technique now universally used—*percussion médiate*.

JEAN-ANTOINE VILLEMIN (1827-1892)

How is it that the name of Villemin is so relatively little known amongst medical men, whereas Laënnec is celebrated everywhere and Robert Koch, the discoverer of the tubercle bacillus, stands on such a pinnacle of fame that he is visible to even the youngest and least interested medical student? Yet Villemin preceded Koch by seventeen years in demonstrating that tuberculosis is due to a specific agent—a *germ* he called it—and that it resembles the zymotic diseases in being inoculable from man or cow to the rabbit and guinea pig and in being transmissible from one infected animal to another in unending series.

Laënnec certainly deserves his celebrity, not merely in being the discoverer of the stethoscope, that instrument cherished by every doctor, but as an unequalled pathological investigator, clinician and writer on the disease from which he himself suffered and died.

And as for Robert Koch there can be no doubt but that, even admitting occasional errors of interpretation, he was still the greatest discoverer, the greatest pioneer on the road of medical truth that has yet been translated to a place in our firmament, where he stands with Pasteur among the immortals. *But why not Villemin?* Villemin, with no better material than was at the disposal of Laënnec, hit on a simple method which was destined to give a new comprehension of the disease tuberculosis; a method which was to confirm the views of Laënnec as to the unity of tuberculosis; a conception of the tuberculous process which was to revolutionize the work of hygienists!

It appears to the writer that Villemin had to bear all the opposition and all the obscurantism that invariably follows a great discovery. He was, at first, almost unheard of in England. Even in 1867, two years after he had *demonstrated* by critical experiments his conception of tuberculosis as an infectious disease, the short announcement by William Budd in the *Lancet*

that he *believed* tuberculosis to be so was received as the remarkable utterance of a new faith! In France, his own country, where he ought to have been supported and assisted from the start, he had to face a solid front of unbelief, even in the Academy of Medicine. In Germany, Virchow threw his weight against him and it was not until Conheim confirmed his observations by his beautiful experiments on the cornea of the rabbit that he began to be taken seriously even in pathological circles. And then came the wonderful discovery of Koch and the demonstration that the *cause* of tuberculosis could be seen, cultivated and used to infect other animals! Is it so very strange that Villemin should be forgotten?

Let us turn back a few—a very few—pages of the past and try to justify, once more, for it has been attempted several times already, his inspired attempts to bring a new truth to its proper focus on human suffering. Villemin, as we are told by one of his later biographers, Pierre Astruc, was born on January 25th, 1827, at Prey, a small town or village in the Vosges, five kilometres from Bruyères. His father, Jean-Baptiste Villemin, was a farmer, and far from wealthy. His mother, born as Marguerite Demangeon, had probably the hardest task of all in guiding our hero through his boyhood and youth, for his father died when he was but ten years old. He owed to an uncle, Claude Nicholas Villemin, the monetary assistance necessary to start him on his studies and in his first steps towards a university career at Bruyères where he studied under Professor Bouley. He gained a little money, too, by making drawings of plants for a certain Dr. Mougeot, a naturalist and mycologist, for he had the artistic sense and could sketch well.

At first he prepared to be a teacher but it became necessary for him to go through his period of military training and, called up to the 14th Regiment of the Line, he decided to make his military life a permanency and started to study for the non-commissioned officer's school. The attempt, however, did not meet with immediate success. At this moment it is thought that his colonel advised him to try to become an army doctor. Whether by this officer's advice or by other means he was taken off the strength of the 14th Regiment and attached to the 37th,

then in garrison at Strasbourg, where, sixteen months after his entry to the Army, he entered for instruction at the military hospital on the 17th of November, 1849.

Villemin became an army doctor in the month of August, 1853, and was appointed an *aide-major stagiaire* at the School of the Vâl de Grace in November of that year. In 1854 he became *aide-major*, 2nd class, to the 7th Regiment of Horse Artillery. But his personal tastes and abilities were inextinguishable. His artistic powers had re-asserted themselves during his professional training. Morel's *Précis of Histology*, illustrated by Villemin, appeared in October. The book on histopathology by Morel and Villemin followed. In the same year he accepted an appointment as assistant in the physiology department in the school at Strasbourg; a year later he was appointed a *professeur agrégé* at the Imperial School of the Vâl de Grace and he started to teach clinical medicine there. Amongst other publications from his pen appeared *Le Tubercule au point de vue de son siège, de son evolution et de sa nature* (1861), and *Recherches sur la Vesicule Pulmonaire et l'Emphyseme* (1866). At the Vâl de Grace, too, he submitted to Godelier and Michel Levy, his chiefs for the time being, his desire to study tuberculosis which, in words quoted by Astruc: "il pressentait qu'elle devait se propager à la manière de la morve." This utterance is of the greatest importance. Throughout all his subsequent studies he was filled with the idea that, seeing that glanders was transmissible by inoculation, a disease so very much akin to his own subject, tuberculosis should prove to be the same. As a result of his solicitations accommodation was found for him at one end of the bacteriology department: "without equipment or instruments and with three or four guinea pigs and rabbits between a few planks nailed to the wall." Here he set to work and it was from such limited accommodation and facilities, improved, no doubt, by his own exertions as he settled down, that the whole astonishing volume of his future work was to emerge in due course.

I shall not attempt to follow Villemin's example in keeping his experiments to the last as is done in his great work on tuberculosis, *Etudes sur la Tuberculose*, but shall consider them

first in a short paper that deals only with those of his findings that put him so far above even the best writers of his time. But it is necessary, nevertheless, to study, even before approaching his experiments, the bent of his mind when he began them. "Action and reaction"; that was the theme of his thoughts. "Every vital reaction has its cause outside the anatomical elements that exhibit it." Villemin puts it thus to himself: "I am forced to the belief that the congestion of the closed follicles of the intestine and the mesenteric glands in typhoid fever, the formation of buboes in plague, . . . etc. . . . are all of them phenomena due to the reaction of the sick organs against the action of the aetiological agents of these affections. . . . People have given these causes the name of *virus*. . . . Thus in smallpox a mass of variolus tissue must have made its impression on the epidermis wherever pustules are formed. And, let us say in anticipation, in tuberculosis, a general disease if ever there was one, the myriads of granulations that are scattered closely throughout the lungs, pleurae, peritoneum, etc., could never have been formed except under the direct irritation of an agent of provocation which must have been carried in a state of extremely fine division into all the organs." It was in this frame of mind that Villemin commenced his experiments. He was saturated with the belief that tuberculosis, glanders and syphilis, though due to viruses peculiar to each, were all three to be associated as diseases of an extreme degree of comparability. "Having demonstrated that tuberculosis . . . has all the appearance of a general affection resulting from a morbid agent which affects the whole body, that it develops and is propagated under the conditions common to zymotic diseases, that it has the most marked analogy with glanders-farcy, . . . we have been led to suppose that it would be inoculable like its congeners. . . . The experiments which I shall now describe (*que forment le sujet de cette étude*) have fully confirmed this belief as one can judge from noting them well!" So he writes about the enquiries which follow.

March 6th, 1865, was a great day for Villemin. Upon it he started that series of experiments which was to carry him forward to the final solution of his great problem.



REUTLINGER Phot

JEAN-ANTOINE VILLEMINE (1827-1892)

“ We took two young rabbits in excellent health, still feeding from their mother and living in a cage raised above soil-level and comparatively sheltered. Into one of these rabbits we introduced into a little subcutaneous wound behind each ear two small fragments of tuberculous material and a little of the pus-like fluid from a pulmonary cavity from a phthisic who had died thirty-three hours earlier.” On the 30th of March and on 4th of April the inoculations were repeated. After three and a half months—that is to say on June 20th—Villemin killed it and found a number of tubercles on the peritoneal cavity, along the greater curvature of the stomach and in various other portions of the body, “ some showing a little yellow point in the centre.” The lungs were full of large tuberculous masses apparently formed by the agglomerations of several granulations. The brother rabbit, which had shared all the conditions except the inoculations, was also killed and found free from tuberculosis. The experiment was repeated in six series. In all of them the inoculated rabbits showed tuberculosis while the uninoculated “ controls ” were free from it. Very few of these rabbits died, however, and the majority followed the first in retaining their health for the three or four months of the experiment. Their bodes were infected with the disease but they did not die of tuberculosis although Villemin thought them all doomed to a spread of the lesions. To me, in the light of many such experiments, it seems certain that if they had been left for a year or so the tuberculosis, instead of increasing, would have very largely disappeared. A few rabbits, inoculated with human tuberculosis, usually die fairly quickly of allergic symptoms, but the majority gradually recover. Villemin was fortunate in killing the animals with which he was experimenting at about two months after inoculation when they were still showing the results. Now let us see what happened when he made use of the cow as the source of tuberculosis for his rabbits instead of man. “ On the 14th of February we inoculated two rabbits with tuberculous material coming from a cow set aside for dissection by the veterinary students at Alfort. Three days later one of the two rabbits was destroyed by a dog. The second, “ of remarkable beauty and vigour,” rapidly became thin and, towards the

end of April, got diarrhoea, fell into pronounced marasmus, and "was on the point of dying when we killed it on April 30th." The animal was found riddled with an acute generalized tuberculosis. Villemin, in fact, always found that his rabbits died rapidly when inoculated with bovine tuberculosis. As Stanley Griffith and Cobbett found, a young rabbit will die within five or six weeks when inoculated even with the smallest dose of bovine bacilli although it will survive with twenty times the amount of a human strain. The same phenomenon was observed by Villemin. "It is remarkable," he writes, "that none of our rabbits, inoculated with human tuberculosis has presented a tuberculosis so rapidly and completely generalized as that obtained by inoculation with the tubercle of the cow. . . . This would suggest that tuberculosis of bovine origin inoculated into the rabbit shows a greater activity than that of man inoculated into the same animal." How very near he came to making the discovery which redounded to the credit of Theobald Smith in 1897! But he just missed it, full as he was of the idea, opposed by Colin, that tuberculosis from human and from bovine sources was still *tuberculosis*. He goes on: "This makes one suppose that, like all other virulent substances, tuberculosis acts with the greater intensity when there is a physiological affinity between the creature that furnishes the virus and that receiving it." This explanation of the facts, obvious to Villemin in the light of his experiments, though erroneous, was about as complete as was possible at a time when the bacillus of Koch was still unknown. As a matter of fact it was nearer to the truth than the opinion of Robert Koch himself when he first isolated the bacillus and found that the human and the bovine were apparently the same. It has lately been realized that certain types of cattle exist which are remarkably resistant to what we now recognize to be the bovine type; a matter which has been demonstrated by Soparkar and others; and this fact makes it obvious that physiology cannot quite settle the question.

Villemin now thought his work sufficiently far advanced to be brought to the notice of the Academy of Medicine and read a thoroughly interesting and, to us nowadays, a thoroughly convincing account of his experiments at a meeting of this

body in 1867. To his surprise and, be it said, to his deep distress and mortification, it was received with only tepid enthusiasm and with frank disbelief by the older and more distinguished members. Pidoux, for instance, who was on the point of receiving a donation of 10,000 francs for his contributions to science, was definitely against anything which might militate against the theory of "diathesis" in tuberculosis. That must have been a sore criticism for Villemin! Of "diathesis," he writes: "Ce que nous venons de dire s'applique entièrement au mot 'diathèse.' . . . Le mot diathèse, dit Forget, c'est l'inconnue, c'est l'expression de ce *quid desiratum* qui reste au fond du plus grand nombre des problèmes médicaux; c'est un hieroglyph, un X algébrique apposé par la nature au fronton de l'édifice médical." But he must have been cheered by the remark of Bouley that "Il est incontestable que tous les lapins que touche M. Villemin avec sa lancette deviennent tuberculeux; il faut bien admettre que c'est lui qui les tuberculise."

In the end, Villemin was thanked for an interesting paper and the matter allowed to drop. It is as well, perhaps, that the British Government attempted to verify this highly important matter and that the Privy Council arranged for Dr. Burdon-Saunderson to visit France and to follow up the experiments. With Dr. Simon of the Local Government Board he endeavoured to do so after his return and together they inoculated at least fifty-three guinea pigs and produced in them, or rather, in fifty of them—for three appear to have been exempt—the same signs of tuberculosis that Villemin had found in his animals. They reported that material taken from the known tuberculosis of men and transferred to their guinea pigs had produced lung tuberculosis in all except three.

The kind of infection produced in the liver, the glands, the spleen, the kidneys and in the peritoneum were all faithfully described. The disease was transmitted to other guinea pigs from those first infected from man. Nothing could be more complete. And yet Burdon-Saunderson hesitated! Some inner devildom persuaded him to try one more experiment with setons of "unbleached cotton" in the shoulders of two guinea pigs. One of the animals, though affected at the site with an

inflammatory thickening, was found, otherwise, quite well. The second, however, was more generally affected. It died on the 15th of March and the lungs, liver, spleen, and the neighbouring lymphatic glands were found to be riddled with what were presumed to be miliary nodules of tuberculosis. "In this case, therefore, a pathological process which had originated in traumatic subcutaneous suppuration resulted in lesions which were characteristically tuberculous."

Simon, in his report, spoke as follows: "First, M. Villemin's fact is established as unquestionable: certain of the lower animals, if inoculated from the human subject with the morbid products which are called 'tubercular,' will in consequence develop . . . a disease which is indetical or nearly identical with the so-called 'tubercular' disease of man." But he goes on: "A slight open wound such as that of a seton run beneath the skin" of certain rodents "is not, as in man, an affair of simple irritation which ceases when the irritant is withdrawn, but is capable of being the first step in a series of changes which gradually infect the creature's whole body with imitations of the human 'tubercular diathesis' and thus at last create such 'tubercular' disorganizations as necessarily destroy life."

This one animal, infected either with what is now called "pseudo-tuberculosis rodentium" or, by some accident, with tuberculosis itself, was enough to turn these two intelligent observers from the truth which they had so well demonstrated and to plunge them into a vortex of excusable error! *Why did not they try infecting their fifty-three guinea pigs with "setons of unbleached cotton"?* *Why indeed!* But the faithful observations of these two Englishmen served to confirm in the most ample manner the findings of Villemin and, apart from the misunderstanding above described, were the most absolute verification of the latter's work that could be desired.

There is another question; was Villemin the true originator of the infectivity of tuberculosis for other animals by inoculation from human tuberculosis lesions? Koch says not. Whether by some international jealousy or by some twist of the mind against one who had demonstrated the ready transmission of infection

to rabbits seventeen years before, he took up the cause of a German Professor, P. F. H. Klencke, who had already experimented with a rabbit in 1843. After a notice of his work, Koch summed up by expressing it thus : " Klencke muss demnach als Entdecker des experimentellen Tuberkulosen bezeichnet werden." Now this is a very important inference to draw from the one short paragraph in Klencke's description which appears to be all the record available of his observations on a single rabbit. We translate Klencke's words as follows : " The tuberculous cellules behave like those of cancer." It is to be noted that Klencke was out to show that tuberculous " cellules " behaved like cancerous ones in reproducing the disease when intravenously inoculated! " They can be reproduced when transplanted. A demonstration of this fact is as follows: Tuberculous cellules, previously prepared and shown to my assistants under my Scheik apparatus, were introduced into the jugular vein of a rabbit which, killed six weeks later, showed marked tuberculosis of the liver and the lung. This rabbit then served to inoculate a rook but without any hope from or any success of the inoculation ? "

Now this was clearly an attempt to demonstrate the similarity of behaviour of " tuberculous cellules " to the cellules of cancer; not an experiment to prove the infectivity of tuberculosis! And it is doubtful whether it succeeded in its secondary aim to reproduce tuberculosis. What the nature of the " extensive tuberculosis of the liver and of the lung " may have been remains shrouded in the unknown and the unknowable. It may have been and very probably was tuberculosis—but who shall say? There is no close examination, either macroscopic or microscopic, to lead the reader to a conclusion and the inoculation of the rook from this mass of tissue could not have led to any result. Rooks do not " take " the inoculation of tuberculosis of human origin at all. There is no mention of the *kidneys* of the rabbit and yet they should have been far more prominently affected than the liver if this had been an ordinary infection with tuberculosis. There is no way of knowing whether the experiment produced tuberculosis or not. Clearly one cannot accept as final the conclusion of Klencke. To pronounce

the lesions tuberculous would require histological examination and the inoculation of other rabbits or guinea pigs from the originally infected animal, as was done by Villemin in his experiments before he attempted to bring his results to the notice of the profession. Klencke was out to prove the entire similarity of the behaviour of cancer and tuberculosis and he succeeded well enough to be satisfied with the result. We can be sure that he would have been greatly surprised to hear himself described by the future discoverer of the tubercle bacillus as the "Entdecker" of experimental tuberculosis!

No. We cannot entertain the name of Klencke as having demonstrated the infectivity of tuberculosis for the lower animals. Tuberculosis had been often *alleged* to be transmitted by infection. It was thought so by Benjamin Marten, for instance, and by William Budd and by numerous others, Fracastorius included.

But the infectivity of tuberculosis was not only *thought* to be true but was *proved* to be true by Villemin and the justification of his opinion is framed in everlasting memory in his great book, *Etudes sur la Tuberculose*, which I recommend to students of the history of tuberculosis.

Villemin practised inoculation of tuberculosis into various animals; the dog, the cat, sheep, goats and birds. Cobbett, in his book, tells us that before the discovery of the tubercle bacillus, "it was commonly believed that tuberculosis did not occur in the dog." He adds, however, that Villemin had shown for the first time, in 1868, that the dog could be artificially infected and that this was confirmed by Koch in 1882. Villemin, as a matter of fact, inoculated six dogs with human material and was able to observe that four of the animals got the disease. Since then various people have typed bacilli from naturally infected dogs; that is to say from dogs brought to veterinary establishments of which about 0.5 to 1% are found to be infected with tuberculosis; observers have found them to be sometimes infected with the human and sometimes with the bovine strain. Stanley Griffith, working for the Royal Commission, was able to show that the dog was not very susceptible but still quite definitely infectible with both types. As the friend of man the

dog must often eat tuberculous sputum and we may be glad, for our own sakes, that he is not very easily infected and that the lesions produced are not usually severe.

The cat, as the playmate of man and especially of the child of man, is a very important animal in this respect. Villemin proved tuberculous materials to be inoculable in two out of three cats. We now know that the cat is, when naturally infected, contaminated by the bovine bacillus; it can only be slightly infected by the human type. Fortunately, too, it withstands infection by feeding, even by the bovine bacillus, and this is satisfactory as feeding with milk is very usual in the relation of man to the feline.

Villemin tried to infect sheep and, in one case in three, appears to have succeeded; but he preserves a sceptical outlook owing to the frequency of *strongylus* infections, affections which, without the bacillus to guide one, are difficult to distinguish from tuberculous lesions. Since then the sheep has been proved to be susceptible to bovine infection of the experimental type, but the disease is very rare in sheep upon the mountainside. He also tried to infect jungle fowl and other birds but, as might have been expected with the use of human germs, completely failed.

Altogether we may say that Villemin's work, done on animals at a period when the tubercle bacillus was still unknown, constituted a very great advance and has been amply confirmed. The sputum being an excretion of obvious importance, Villemin next tried inoculating it. "To keep the sputum for some time in contact with an absorbent surface we took a certain amount, and, having beaten it up with water so as to make an emulsion, we introduced the liquid into a syringe by means of which we carried out a hypodermic injection, placing about 1 cc. in the subcutaneous tissue." This was a departure from the incisions into which he had previously introduced tuberculous tissue or material. The result was immediate. The two animals inoculated both got generalized tuberculosis. This observation is in line with one made by Genaudet in 1867 and it confirms the opinion formed even at that early period that the sputum is all-important in the transmission of tuberculosis.

Villemin also tried what the blood would do. He took blood

from the femoral vein of a rabbit already infected with tuberculosis. This gave rise to tuberculosis in the animal into which it was injected. He took blood from the hepatic vein of a man who had just died of acute phthisis and injected about 1 cc. into a rabbit. This animal got tuberculosis very severely and died of it in five weeks' time. With blood taken from phthisical subjects by means of a "leach" he further inoculated four young rabbits hypodermically behind the neck. The first three failed to develop tuberculosis but the fourth got a slight degree of infection.

Turning to the epidemiological considerations of the disease Villemin speaks as follows: "It is universally recognized that phthisis is particularly common in the capital cities and the popular towns of industry and commerce. . . . In the relative isolation of the country it is rare." He turns to phthisis in the army for evidence of the importance of "contact." "Phthisis in the army! How can one explain the extreme frequency, so much greater than in the civil population, if not by the dwelling together and concentration of the men in barracks? . . . Prisoners, industrial workers, seminarists, members of religious orders, soldiers, etc., have only this in common; the similarity of their housing accommodation. It is this alone that should be invoked as the cause of the frequency of tuberculosis."

He drew an excellent comparison between the arrivals of fresh, young, and healthy men from the country districts, so free from any trace of phthisis on arrival at their town depots and so very liable to it in the next year or so, and the sending of young and healthy horses, free from all trace of *glanders*, to town barracks where they very frequently got the fulminating form of *glanders* infection which is so characteristic of animals of this type.

What the phthisis germ is to the young soldier, the *glanders* organism is to the young and hitherto uninfected horse.

He speaks of the disease, tuberculosis, as something that may easily be missed. "Certain modifications in the intensity and the timbre of the normal bruits of respiration in their absolute or their relative duration . . . allow the experienced observer to recognize with a certain degree of security the presence of

tubercles in the pulmonary parenchyma. But this is not to say that the occurrence of tubercles always reveals itself thus. *We claim on the contrary, that granulations in considerable number, can exist in the lungs without being recognized even by the most conscientious observer.*"

Many things, both pathological and clinical seem to have been known to this extraordinary man.

The subsequent career of Villemin was one of only relative success. His great discovery, admitted at last but only to be universally applauded after his death, was, in the world of medicine, completely outshone by the wonderful achievements of Koch. To actually demonstrate the transmissibility of tuberculosis by *the finding of the germ* was, to the average man and indeed to the thinker as well, a much more satisfying discovery than the demonstration that there was *something* which could be transmitted.

Villemin, with the end of the Franco-German war of 1870, was the holder of the Chair of Hygiene and Legal Medicine at the Vâl de Grace and was promoted, in 1873, to succeed his old colleague and chief, Godelier, as Professor of Clinical Medicine. His experiences during the war had been very trying. He had sent his wife and son away to a small property of his near Agen, and he remained without news of them during the siege. Then came the end, and though he received a growing recognition, the first place in the study of tuberculosis, until then the glory of France, was destined to pass gradually to the rival nation and to become the attribute of the Germans. Finally, in 1882, Robert Koch became known to the world for his almost magical discoveries and shone forth as the finder of the causative agent of tuberculosis, to be known ever after as Koch's bacillus. All this must have been gall and wormwood to Villemin. It is touching to read his letter on the subject to Pasteur in 1887; so much so that we quote it as follows:

"I do not hope to gain a place beside you but—you will see that I am less modest than I appear—I have been so much discussed, so often attacked, that I suffer a certain amount of distress in thinking that the leading scientific academy still gives, at least, a sort of toleration to my former enemies.

You must well remember the discussion and controversies of 1867 and 1868 both in the learned societies and in the press of France and other countries. Twenty years have passed and all this has become merely ancient history! Koch's bacillus, of which the Germans are so proud, has obscured the memory of what the French scientists had already accomplished. Koch will enter the Académie des Sciences through widely flung doors, in the triumphant way that has made a conquest for him of all the honours of his country."

There is a quality of bitterness in this which it is easy to understand though impossible, perhaps, to sympathize with. So great was the accomplishment of Koch that it was useless, though perhaps natural, for Villemin to belittle it. After all it was merely the placing of a crown upon what Villemin himself had first discovered; the contagious nature of tuberculosis. The two men were destined to be complementary the one to the other. Had they not been separated by the anguish of war and the antipathies of race they would have stood together in the history of tuberculosis as the two discoverers of component parts of the same glorious edifice. Let us honour them both, Jean-Antoine Villemin and Robert Koch, destined as they were to hew out between them the secrets of the most terrible of human diseases; to demonstrate to all men the infective nature of tuberculosis!

PART III

Two Remarkable Figures in the History of Tuberculosis



EDWARD LIVINGSTON TRUDEAU (1848-1915)

IN attempting to tell briefly the story of Trudeau, I am fully aware that I am going out of the epoch which I had reserved to myself for this little volume; but it seems to me to be impossible to leave out mention of the direct legacy of Robert Koch as imparted by him, all unconsciously, to the New World; and yet there can be no doubt but that the seed sown in America has had the very richest harvest and that the next historian of tuberculosis will find there those who have most truly implemented with their full life-energy the great discovery of that far-from-simple germ, the bacillus of tuberculosis. I fear that I must consign to that future historian the stories of Theobald Smith, Edward R. Baldwin, Leroy S. Gardiner, Allen K. Krause, George E. Bushnell, Eugene L. Opie, Arnold Rice Rich and a score of others; but they all represent the present age and are not therefore within my period. I must reluctantly bid them adieu and wish them a much better recorder than me to send their names, shining so brightly, down to posterity.

But I feel that the story of Trudeau, although so close to our own time must take its place with those of the great historical figures of the past; must arrange itself with all those who *originated* new forms of treatment; must establish how very widely as well as deeply the doctrine of Koch has penetrated into the world of medicine.

Trudeau was born in New York on October 5, 1848. He came of a strictly medical ancestry. His father, Dr. James Trudeau, was, however, not only a doctor but a soldier. When the Civil War broke out he became an officer in the Southern army and was wounded and brought back to New Orleans where he recovered, more or less, and was able to practise his profession before his death. "The love of wild nature and of hunting was a real passion with my father" says Trudeau in his autobiography, "a passion which ruined his professional career in New Orleans for he was constantly absent on hunting

expeditions." This passion, which led him to live for two years with an Indian tribe, was transmitted to his son as something to ameliorate the agonies of a death which then seemed so near; so near until he made his great discovery that tuberculosis is not necessarily a fatal disease and that one can live on and prosper in spite of it, provided that the activities of life are kept in bounds by the great natural remedy, rest, to which all tuberculotics must submit if they want to survive! "This same love of wild nature and hunting was reproduced in his son," says Trudeau, "for when stricken with tuberculosis in 1872 it drove me, in spite of all the urgent protests of my friends and physicians, to bury myself in the Adirondacks . . . in order to lead an open-air life in the great forest, alone with Nature and those who were dear to me." Trudeau's mother was a Mademoiselle Céphise Berger, the only daughter of Dr. François Eloi Berger, a French physician who came from a long line of doctors; doctors of the hereditary kind who had gone back for as many generations as could be traced. Shortly after Trudeau's birth his mother and father separated. There had already been two children, first a daughter and then a son; the third was Trudeau who, with his brother, accompanied his mother and his grandparents to France, he being then at the age of three. His sister accompanied her father to New Orleans. Thus the family was broken up, young Trudeau staying with his grandfather, Dr. Berger, who had now retired from a large practice in New York and had returned to his native Paris. Shortly after this, Mrs. Trudeau obtained a divorce from her husband and married a French officer, Capitaine F. E. Chuffart, with whom she lived until her death in 1900 at Fontainebleau.

Thus Trudeau came to be educated in Paris and remained there from 1851 till 1865 when the whole family, Dr. Berger and his wife and the two Trudeaus, returned to New York. During his fifteen years in Paris he was sent to a school where, according to his diary, the whole influence was bad for the formation of a boy's character. "Cowardice, lying, cheating and deception of all kinds" were in vogue among the boys and, as he states, little frowned upon by the masters. How strange that from such an environment should spring one of the bravest,

most truthful, most sincere, of men; perhaps it was just the difference of race that led to it; an American amongst a number of French scholars who might behave as they liked but were not to be followed by him!

The home of Dr. Berger, in Paris, was in the Rue Matignon and was a typical French apartment house with a *porte cochère* entrance where carriages could drive up and turn round or could wait in the large courtyard at the back. The first floor was always hired by the French Government for one of the generals on the staff of the French army; at this time no less a person than General Bazaine. Trudeau's grandfather, too, was chosen for the favour of the Emperor and was decorated with the Legion of Honour, which led to his wearing the ribbon in his button-hole; but, when asked what the ribbon was for he always said to his grandchild: "Pour faire parler les curieux, mon enfant."

The children did not however forget their American nationality and there were ardent passages between the Northern and the Southern boys, as shown by battles between the rival vessels on the local ponds, where the Southerners did not always get fair play from the more numerous Northerners, of whom the young Trudeaus were fair specimens. Edward, in fact, fired his catapult after a well-known Southern diplomat and hit him in the back, for which he was well punished; or rather his coat was! "A heavy hand seized me by the coat collar and a large umbrella came down on my head and shoulders with a resounding whack. Quick as a flash, I wriggled out of the coat and ran. As I turned to look back I saw the excited gentleman, purple with rage, beating my coat with his umbrella and heard his suphurous remarks to the boys who, too awed to laugh this time, kept a respectful distance." Instead of being thoroughly whipped for this, he was taken by his grandfather to the embassy a few days afterwards and introduced as the lad who had "shot Mr. Slidell" and was complimented on his performance—but he never got back his coat!

On his return to America, now at the age of seventeen years, he was charmed to find that the relations between the sexes were much freer than had been the case in France. "To find myself all at once thrown intimately and unrestrictedly with

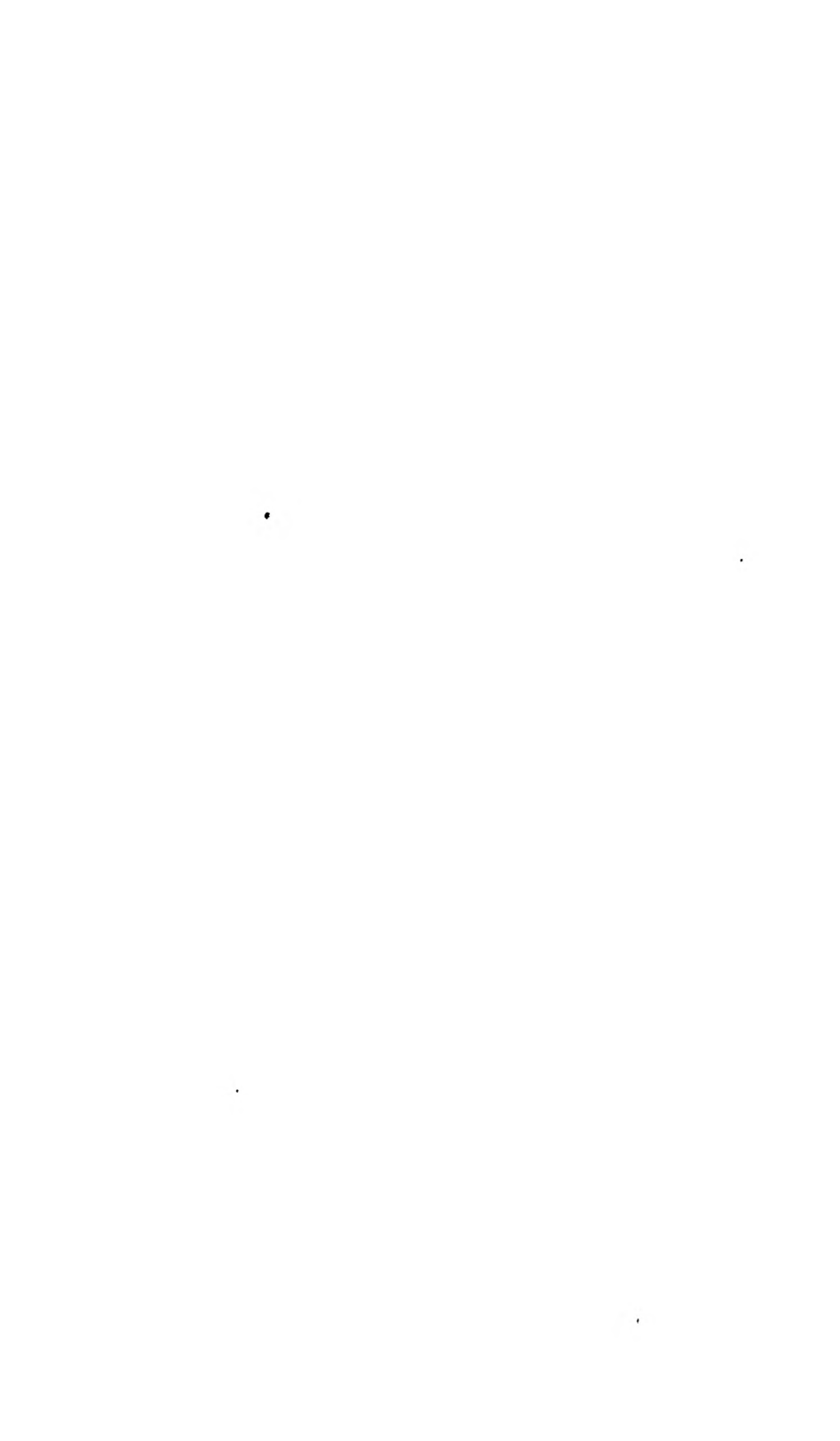
my girl cousins and their girl friends, in winter to walk and ride and dance and skate with them and, in summer, to drive and sail and row and swim and dance again with them, was a new revelation to me, and I think I made the most of my opportunities." Here, on a trip to Nyack, he first met his future wife. "my cousin, Minnie Aspinwall, and I had arranged that we should go up to Nyack together that day. When I called for her on Eighteenth Street I found her talking to a tall, very slender young woman, dressed in black, whom she at once introduced to me as Miss Lottie Beare." He carried her travelling bag up to his cousin's house for her and thought that she was rather cold and reserved, but, as he says, it was the tall slender girl in black who soon inspired him "with a love which made me give up all the wild mode of life into which I was fast slipping in New York and work for three years to obtain a medical degree and for a lifetime to try to be worthy of her."

Trudeau then thought he would enter the Navy and it was all arranged that he should enter the old ship, *Constitution*, as a midshipman, when his brother, to whom he was devoted, began to develop the consumption from which he very shortly died. "I was shocked at his appearance" says Trudeau, "and, when he told me the doctor said he had consumption I at once threw up my appointment and returned with him to my grandfather's house . . . We had no trained nurses in those days and I took entire care of him from the time he was taken ill in September until he died in December 23, 1865. We occupied the same room and sometimes the same bed. I bathed him and brought up his meals to him and, when he felt well enough to go down stairs, I carried him up and down on my back . . . My sister and grandmother often sat with him in the day-time and allowed me to go out for exercise and change, but he soon became very dependent upon me and I had to be with him day and night . . . I remember that, during the last week he lived I had to drink green tea every night in order to keep myself awake but I held out to the end . . . This was my first introduction to tuberculosis and to death."

This devoted and intimate contact with a brother to whom he was attached not only introduced him to tuberculosis but



EDWARD LIVINGSTON TRUDEAU (1845-1915)



must have infected him with the germs of a disease which was, within a few years, to reduce him from his splendid health and compel his transfer to very different and unexpected surroundings; to put out all his hopes for a medical career in or near New York; to send him, despairingly, to the woods and lakes of the Adirondacks and finally, for such is the curious influence of tuberculosis upon exceptional men, keyed up to struggle with and overcome it, to an almost complete mastery over the disease and a way of cure which was to help many to survive in the face of it!

For the time, however—for such is, again, the nature of this dreadful malady—he was to regain all his strength and energy, to try several billets without success, to quarrel with his grandmother on whom he was completely dependent, to sail very close to a life of dissipation and frivolity; and eventually to choose, almost naturally, the hereditary profession of medicine which was evidently “in his bones.” At the same time, it was an almost incredible event to many of his intimates. “So little faith did my friends at the [Union] Club have in my doing any work at all that I remember when my decision to study medicine was announced to a group of men at the club, Mr. Dan Moran said: ‘I bet five hundred dollars he never graduates,’ and no one was found to take the bet!”

Nevertheless, he persisted, matriculating and going to the College of Physicians and Surgeons, not a very imposing building on the corner of Twenty-third Street and Fourth Avenue, and following the lectures on the practice of medicine, surgery and pathology, the latter taught, at that time, by the Chair of Medicine, as a side issue.

The theories as to the causation of disease were, of course, discussed, but the essential exciting causes, not being yet known, remained theoretical. “This was true of Tuberculosis” says Trudeau. “Dr. Alonzo Clark taught that it was a non-contagious, generally incurable and inherited disease, due to inherited constitutional peculiarities, perverted humours and various types of inflammation, and dwelt at length on the different pathological characteristics of tubercle, scrofula, caseation and pulmonary phthisis.”

It is very interesting to find this solemn nonsense being taught up to the very moment when the minds of men were to be turned suddenly towards the great light by the works of investigators like Villemin and Conheim and, later, by the findings of that wonderful discoverer, Robert Koch himself. Truly medicine was to unfold itself in a moment; to become quick and to slough off the mantle of ignorance that had covered it for so long; to raise itself from a suspended art to a progressive science in the twinkling of an eye.

Trudeau's summing up of his state of health at that time was as follows: "Though slender, I was quite athletic, very active; and had wonderful endurance."

This plenitude of strength was once put to the test at the instance of his relatives and great friends, the Livingstons, who made a bet that he could walk from Central Park to the Battery inside an hour. One night, at midnight, he started and succeeded in walking the distance in forty-seven minutes but it was too much for him and he felt ill for a long time afterwards. This feeling of illness was, perhaps, one of the earliest manifestations of the disease which was destined to overtake him. It was accompanied by the formation of a cold abscess which had to be operated upon several times before it healed. In those days, however, as he says, "the relation of such cold abscesses to tuberculosis was not understood" and nobody ever suggested to him that it might be an early symptom. Had anybody done so it might have saved him great trouble by an early treatment of rest and so forth. Many years afterwards, when asked to speak on tuberculosis in New York to Dr. Walter B. James's class of students, Trudeau said to them that "if a patient came to them with a dry pleurisy, blood-spitting or a cold abscess" they should suspect tuberculosis; excellent advice.

All this time Trudeau was deeply in love and ready to perform almost anything in the way of personal sacrifice to please the girl of his choice. He determined to buy a good horse for Miss Beare for her birthday and, at the express desire of Patsy, a wide-awake young Irish groom of the Livingstons, ended by purchasing, for three hundred dollars, a beautiful bay with black points which had originally been priced at a thousand! He rode it over

to his beloved with the greatest pride—but, trying it rather highly at the end, noticed that the horse was lame, shoulder lameness, and realized that he had been “done.” He had to walk the horse up to her father’s rectory and, instead of showing her its beauties and then giving it to her, to confess that he had been cheated. Miss Beare, however, was “so full of kindness and sympathy” that she fully made up to him for this bitter disappointment and, shortly after, the splendid looking steed having been sold for seventy-five dollars, he was able to buy “a common useful animal” instead. Whether this episode hurried up matters or not, he was, shortly after, allowed to announce his engagement, and he and Lottie Beare were duly married on June 29th, 1871, after which they were driven to New York in a borrowed coupé, by Trudeau’s boy, in livery, and everything was as much for the best as it could possibly appear to be! “I was certainly carefree in those days and the horizon and the future were brilliant and rose-coloured.”

Just how “rose-coloured” may be imagined from a little story which is as follows: “About this time I received unexpectedly a payment of twelve hundred dollars from my grandfather’s estate . . . When the money came in unexpectedly it seemed to me the most natural and satisfactory use I could possibly make of it would be to buy Lou’s turnout with it.” “Lou’s turnout” was a pair of little mares which Lou Livingston used to drive in a light Brewster trotting wagon; and which Trudeau now took over.

So he and his newly acquired wife drove to New York with all his little bequest of twelve hundred dollars trotting below them in the form of two brilliant little mares! What could be better than this? Hadn’t the money come to him and gone from him to make Lottie happy and proud? “I imagined” says he, “with what disapproval her staid friends and family must have looked on when she appeared in an up-to-date trotting rig with me; but as the little mares picked their way through crowded Fifth Avenue, and later when we flew up Harlem Lane at a two-forty gait, no one could have been prouder or happier than I was!”

There was a small establishment called the Stranger’s Hospital

which had just been opened at the corner of Tenth Street and Avenue A, and for the positions offered Trudeau became a candidate. He was very pleased to hear, a few days after sitting for his examination, that he had passed the best and had the choice of which appointment he liked. He chose the post of house physician which only lasted for six months. This position meaning, as it did, constant hard work, led to his becoming very thin and worn-out at the end of his time there. "I must have had excellent resistance to have kept so well so long, under trying conditions, after the positive evidence of tuberculous infection given by the cold abscess eighteen months before." So he speaks of this time and it is evident that he looked back on it as a very "close shave" when he knew himself to have had a tuberculous condition for so long. But all this mattered very little to him then and he left the hospital, took his fortnight's leave at the end of his time there and got himself married, as we have said, on the 29th of June, 1871. He and his wife then sailed for Europe, going first to London and then to Paris to revisit all the old scenes, after which they went on to Switzerland and to Germany. "While in England I had some swelling of the lymphatic glands on the side of my neck, but so ignorant were we about the mechanism of tuberculous infection at that time that this symptom gave me no alarm. . . . The second warning of tuberculous infection went as unheeded as the first." On returning to America they took a little cottage at the gate of the estate of their friend, Mr. Douglas, from whose home they had been married and there spent a very happy year trying to found a practice on Long Island; but Trudeau found life in the country too limiting as a means of advancing in his profession and so, a year after his return from Europe, he moved to New York.

He succeeded in securing a three years lease of a suitable house, No. 8 West Street, at a price which he could manage, and he had not been settled there more than a few months when he was offered a partnership by Dr. Fessenden Otis with every prospect of succeeding him after a little time, an offer which he gladly accepted, and he had a class for diseases of the chest with his friend Dr. Luis P. Walton at the Demilt Dispensary.

Everything seemed to point to a successful future and a position of weight in the medical profession in New York.

And then the blow fell! As the reader will have observed, he had had two outbreaks of what was undoubtedly tuberculosis already, but these had been successfully overcome. Now he began to have attacks of fever, which were put down to malaria, a very prevalent disease at that time and place. He took quinine but this seemed not to do him much good. He felt tired all the time, a very well-known symptom of his condition, but he attributed this to the confinement of a city life. He describes the occurrences which followed in his own words: "One afternoon I was at the Dispensary with Dr. Walton and he insisted that I looked ill and took my temperature. To my astonishment it was 101° F. Walton advised me to go to Dr. Janeway and have my lungs examined but I laughed at the idea." Later, however, the opinion of Dr. Walton came back to him and, partly, he says, to score off the latter and tell him that he was all right, but, as one thinks, with a sort of feeling of suspicion he went to Dr. Janeway and asked for an examination. This was duly carried out and there ensued a silence as Janeway looked at him but said nothing. Trudeau broke this saying: "Well, Dr. Janeway, you can find nothing the matter?" He looked grave and said: "Yes, the upper two thirds of the left lung is involved in an active tuberculous process."

Here it is necessary to quote from Trudeau's *Autobiography* to describe what he felt.

"I think I know something of the feelings of the man at the bar who is told that he is to be hanged on a given date, for in those days, pulmonary consumption was considered as absolutely fatal. I pulled myself together, put as good a face on the matter as I could, and escaped from the Office after thanking the doctor for his examination. When I got outside, as I stood on Dr. Janeway's stoop, I felt stunned. It seemed to me that the world had grown suddenly dark. The sun was shining it is true, and the street was filled with the rush and noise of traffic, but to me the world had lost every vestige of brightness. I had consumption—that most fatal

of diseases! Had I not seen it in all its horrors in my brother's case? It meant death and I had never thought of death before. Was I ready to die? How could I tell my wife whom I had just left in unconscious happiness with the little baby in our new home? And my rose-coloured dreams of achievement and professional success in New York! They were all shattered now and, in their place, only exile and the inevitable end remained."

Even today, with all our experience of tuberculosis, the sudden announcement of a definite diagnosis comes as the signal for a complete change of life. The disease may not, of course, be fatal; in fact the chances are that it will prove, instead, only an interruption of six months or a year in the course of a long life.

This is the result of the impact of X-ray diagnosis on a problem which was, in Trudeau's time, a much more sinister one than now. Yet there were certain facts to take into account if only they had been recognized at the time. Trudeau naturally compared his own condition with that of his brother who had died so rapidly from tuberculosis a few years before. He thought that as his brother had gone he would also go—a speedy victim to an inevitable death! And yet there were points deserving of close attention in distinguishing between the two cases. May I say that a classification of my own, long forgotten but still very useful, if only tuberculosis officers would use it, would have come in in comparing these two cases? The fatal illness of his brother was clearly an A.3.a. case, where A. stands for a case without previous symptoms of the disease, 3. means that the case has already reached the third stage of a highly progressive type, and a. means that the patient is running a temperature. Such cases are, even now, almost invariably fatal. On the other hand the case of Trudeau himself was of a totally different kind. Here there had been at least two warning attacks of a milder type from which the patient had recovered well, showing that he possessed a rich fund of resistance. When he was finally examined by Janeways he went voluntarily to ask the latter to examine him; with the idea that this examination would serve to

exculpate him from Walton's suspicions. And, only then, he learnt that "the upper two thirds of the left lung" were involved. This is the sort of case that, on being admitted to a hospital or sanatorium, is put to bed and gets rid of all the fever and all signs of illness in a few days. Not that the patient is much the better for this change, but at least he *feels* much better and is fit to start the long journey towards health which is necessary for him. In other words, Trudeau was a C.2.b. case; a case with a history of previous attacks with healthy intervals between, a case with at least one lung healthy, a case that could control his fever when placed under favourable conditions. He was, in other words, the kind of case that might, under suitable conditions end by getting much better or even quite well.

This, of course, was hidden from him and he imagined himself doomed. What was his first step? He faced the situation like a man; told his wife all about it, kindly and gently, of course, but with few reservations; took the advice of his doctor to "move South"; lived as far as possible out of doors; and—a very interesting thing in view of the advice of Sydenham, Morton, Marten and other phthysiologists in the seventeenth century, not to speak of Galen himself—took to riding on horseback as a part of his treatment! This was all very bad! The man should have been guided towards a sanatorium or hospital specially for tuberculosis! But where was there such an institution at that time? There was not such an establishment in the whole of America. It was in the womb of the future that Trudeau himself should be the first to discover the value of graded exercise, of rest when necessary, of ample fresh air, of a continuance at a job, even if it were but at the job of hunting, fishing, etc., within the prescribed limits of what a man might do and might not do according to his state, on the one hand, and his desires on the other!

He was to *experiment*, with his own health as the pawn; to fail, try again, to perceive a ray of light, to follow it, and finally, in the light of his successful *experiments*, to found the first great sanatorium of America, a star of hope to the United States and to the whole world.

His first experiment was in the trying-out of the loose advice

given to him to "go South," live out of doors and try horseback riding. All this advice was quite good if only it had included the putting the patient under a well qualified physician to say *how much* the patient might "live out-of-doors" and what limits might be put to the "horseback riding"; but there was no such physician except the patient himself and he was evidently too lenient! He developed daily fever as the result of the riding! Had he even such an early writer as Benjamin Marten by his side, who advised riding but counselled *moderation*, when he wrote of phthisis in the seventeenth century, this downward drift and the daily fever might have been avoided! Trudeau, then drifted back to New York, no better for this first trial.

There he was "allowed and even urged" to take daily exercise with the result that his fever kept up and he lost weight and strength steadily. "It took me a long time to learn," says he "that acquiescence is the only way for the tuberculous invalid to conquer fate."

His friend Lou Livingston, did as much as he could to cheer him up and comfort him in his own way—which generally meant taking him off to see or participate in some sporting event or other. Once, for instance, Livingston arrived with the idea of driving him down to the Union Track, Long Island, to see two men who had never shot at a flying bird trying their hands, for the wager of a thousand potatoes, at pigeons released from a trap. This event was to be followed by a "miss and out" competition for an entry fee of five dollars, for anyone who chose to enter; Lou Livingston was a crack pigeon shot and expected to win something at the latter. "Mr. H. D. Polhemus . . . seemed very sympathetic as to my evident illness and insisted that I take a wonderful gun he had and enter the sweepstakes with the rest"—a typical effort by one unacquainted with tuberculosis—"and I finally consented." The result was that, though more and more fatigued and thoroughly sick of the whole thing Trudeau won four successive sweepstakes, stood the company as much as they could drink, and, after a self-congratulatory notice, "I don't think I ever shot so well as I did on that day, sick as I was," records afterwards that he grew steadily worse and worse and "had to keep to my bed most of the time."

"Of course I was exhausted the next day and had to remain in bed with a high fever." Thus ended the first experiment and it ended in disaster. Trudeau seems to have been pretty hopeless at this stage and he believed that it was all over with him. But his wife was soon to present him with a second child and he decided, against the advice of his medical adviser, to stay where he was until the event should be over. "Our boy was born on May 18, 1873, and a week later, Lou Livingston and I set out for Paul Smith's." His friend Dr. Walton, was of great use to him and of great comfort to his wife. After his wife and the babies had been sent to her father's care at the Rectory at Little Neck, Dr. Walton paid her regular visits and kept Trudeau informed of how they were getting on. "His friendly watchfulness of my dear ones and his letters were of greatest comfort."

"I was influenced in my choice of the Adirondacks only by my love for the great forest and the wild life and not at all because I thought the climate would be beneficial in any way. The place was then only visited by hunters and fishermen and was as Trudeau says, a rough inaccessible region with an inclement and trying climate. But he was determined to go there and nowhere else. "If I had but a short time to live I yearned for surroundings that appealed to me and it seemed to meet a longing I had for rest and the peace of the great wilderness." And thus he began his second experiment of which he hoped for little except the undisturbed quiet of a remote, if otherwise unsuitable, place to linger in until death should end the scene.

And yet it was this second experiment which was to succeed! Things began ominously. The journey to Saranac was a terribly trying one for a man stricken with tuberculosis. Lou Livingston, who, like a faithful friend, accompanied him, several times counselled his return; but Trudeau was adamant and so they travelled on together. From Plattsburg it was impossible to start forward at once. "I had raging fever all day, went to bed at once on reaching the Fouquet House, and was too ill and weak the next morning to attempt the long trip into the wilderness to Paul Smith's." On the third day, they reached Ausable Forks, and from there to Paul Smith's they had to face a forty-

two-miles drive over a rough corduroy road. "While I was resting Lou hired an old fashioned two-horse stage-wagon, put a board between the seats and with a mattress and a couple of pillows arranged me so that I could lie down comfortably."

This Lou Livingston must have been a wonderful friend to him on this terrible journey. It is touching to read of all the care he took of his charge. He must have seemed the agent of heaven itself to the poor helpless man whom he had undertaken to see through his journey into the unknown.

How easily might such a journey have turned the scale against a patient suffering, as Trudeau did, from pulmonary tuberculosis; but, with it all, there was a chance at least that he might weather it and reach his journey's end without the catastrophe that must have resulted had he been, like his brother, an "A.3.a." case. But he had already lived down his cold abscess and his attack of cervical glands and had regained almost perfect health and, though this was a far more serious bout, might he not still get the better of it? Such a wonder was yet to be accomplished, as we shall see, but it was evidently "touch and go" with him and it was only his personal courage and the unknown resistance to his disease that carried him through.

For there were better times in store for him and the start of his successful experiment was about to take place.

"The sun was just setting as I caught sight of the great pines around Paul Smith's and in a minute we were driving up to the door of the hostelry, a swarm of guides and fishermen were clambering off the steps and the horse-block, and many hands extended in welcome. Fred Martin, Mrs. Paul Smith's brother and one of the most splendid, sturdy specimens of manhood I have ever seen, was about to give my hand a squeeze that would, no doubt, have finished me, when I whispered to him that I was sick and wanted to be carried up to my room. He picked me up as if I had been an infant and went up two flights of stairs, two steps at a time, opened the door of the room I had occupied before, and put me down on a bed with a pained expression and the comforting remark: 'Why, Doctor, you don't weigh no more than a dried lamb-skin.'"

The first thing was a change in mental outlook. "During

the entire journey I had felt gloomy forebodings as to the hopelessness of my case, but, after the magic influence of the surroundings I had longed for, these all disappeared and I felt convinced I was going to recover."

Was this the "*spes phthisica*"? or was it more a real kind of hope, engendered by that very resistance of which I have spoken? Who can say? That he possessed that resistance was a fact at least, and a potent source of recovery.

"How little I knew," he goes on to say, "as I shook hands with the great strong men who came up to my room that evening to say a word of cheer to me that, forty-two years later, most of them would be dead and that I should be still in the Adirondacks and trying to describe my first arrival at Paul Smith's as an invalid."

Forty-two years! Nearly half a century! And it must be admitted that life, for him, was often broken up by attacks of fever which, however, could always be compensated and stopped by a few days rest in bed! Still it must be admitted that he was an extreme case of adjustability to his surroundings; to the carrying on of sport and, later, work, to the limit and then the temporary giving up to his fever and his bed!

"I slept well and woke full of hope and anticipation and interest in my new surroundings. The first thing I did was to secure a guide, and Warren Flanders was engaged by me and George Martin by Lou Livingston." That looks the right kind of response to his surroundings, *provided that the guide should be so far controlled as to make his "guiding" sufficiently attenuated to suit a very sick though very keen, man.* As to this, we shall see how Trudeau arranged this desirable result.

"Warren Flanders came to my room after breakfast and told me that he had fixed the boat 'comfortable' with balsam boughs and blankets so that I could lie down in it; had put my rifle in, and if I felt up to it, we would row down the river to Keese's Mill 'kind of slow' and see what we could see." What could be better? It was a splendid June day, the sky and water blue, the trees resplendent in their spring foliage. He describes how he lay comfortably on the soft boughs in the stern of the boat with his rifle in reach. "My spirits were high and I forgot all

the misery and sickness I had gone through in the past two months."

It will be evident from these lines that the second experiment was, so far, meeting with an almost magical success. Trudeau had hit upon a plan by which he could indulge in the kind of sport that his heart yearned for and at the same time be rested, quiet and easy, as any man should be who wants to keep his tuberculosis under control.

As for the adventure on which he had set forth, those interested in sport will desire to know the end of it and everyone will be glad to know that Trudeau kept up his rest-treatment even at the moment of shooting a stag! "The guide kept looking ahead from time to time. All at once he stopped, turning the boat sideways. At a point about two hundred yards away I saw two deer: a buck and a doe were feeding. I never sat up but rested my rifle on the side of the boat and fired at the buck who, after a few jumps, fell dead at the edge of the woods."

A great shot and a great tribute, also, to the self-control of the man who "never sat up but rested the rifle on the side of the boat."

The following months were nothing but pure joy to Trudeau as he gradually felt his returning health and experienced the wonders of his surroundings. A great friend of his, E. H. Harriman, arrived about that time and shared with him the sport and the scenery. "Many were the joyous, beautiful summer days we spent floating over the lakes in our boats, hunting, fishing and camping together wherever we fancied to stop the night."

After a few months of this kind of health-getting Trudeau felt almost well again and, after three weeks with James Livingston during which the latter looked after him in much the same way as Harriman had done, that is to say, by becoming a participator in his sports and pastimes, he felt that he might as well go down and join his wife and the babies at the Prospect House, Catskill, where she and her father were now staying. His description of himself at that time is as follows: "I was sunburnt, had gained fifteen pounds in weight, was apparently in my usual health, and was so anxious to see my little family again that I could hardly wait for the day set for my departure for Catskill."

After a glorious reunion, they all set out for New York and there he tried hard to get his physicians to allow him to start work again but there came a return of the fever which soon showed him the folly of such a course. After a winter in Minnesota, during which he was allowed to "drive and walk and go duck-shooting when he felt equal to it," he developed "some fever most of the time" and was nearly as sick as he had been the year before. The Adirondacks seemed his only hope and so "early in June, accompanied by my wife, the two children and two nurses I arrived at Paul Smith's to my intense joy, for I always loved the place."

He was back again where the second experiment had started so well. Paul Smith's, with the hunting and fishing, all within the limits set by experience, suited him as no place else did. He says of it: "Of late years on several occasions I have been taken to Paul Smith's from Saranac Lake in the Spring so ill that my life was despaired of; and yet little by little, while lying out under the great trees, looking out on the lake all day, my fever has stopped and my health slowly begun to return. . . . Again imperceptibly the fever began to fall, and strength—and with it the desire to live—to return." And, speaking of hare-hunting and fox-hunting, he says words of the deepest truth: "I found, however, I could not walk enough to stand much of a chance for a shot without feeling sick and feverish the next day *and this was the first indication I had as to the value of the rest cure* which I afterwards applied so thoroughly and rigidly to my patients."

Saranac Lake

So much for the Adirondacks and Paul Smith's hospitable care. Trudeau spent a few winters with the latter, brought his mother, who had come from France to see him, to the inn, while his wife and children were, for the moment, away and with her father; had the most adventurous return through the snow with the latter, the two children and their nurse, when he had driven his mother to Malone and undertaken to bring his whole family back with him: "It was in November 1876 that my little family joined me at Saranac Lake" he says "and we have lived

there ever since." This remove had become necessary because the Smiths had had to change their station from St. Regis and the Trudeaus had consequently sought and found a little cottage at Saranac and were now living there. But Saranac was, as may be discovered in the following story, not such a bad place: "That afternoon, after we had unloaded the furniture, I remember I went out with 'Dug' rabbit hunting and killed a big hare ahead of my hound exactly where the station of the New York Central Railroad was built in later years."

"This was the beginning of the now famous health resort known as Saranac Lake, which developed at first through a few pulmonary invalids that Dr. Loomis sent me from time to time to try the effect of the winter climate, and subsequently through my founding at Saranac Lake two institutions, the first of their kind in this country—the Adirondack Cottage Sanitarium and the Saranac Laboratory for the Study of Tuberculosis."

For forty years, then, Trudeau and his wife spent the winters at Saranac Lake and the summers at Paul Smith's, now at the Fouquet House at Plattsburg and free to take guests at the latter place.

In 1876, in the month of May, their third baby was born and in the fall of that year Trudeau started a subscription list for a little log chapel near Paul Smith's, by which "St. John's in the Wilderness" was built. "This" says he "was the beginning of a life-time of begging money from friends, an occupation I have carried on unceasingly and, thanks to the constancy of their friendship, rather successfully for forty years." For Trudeau's faith in his second experiment was growing and he was becoming more and more eager to try out the magic combination of "rest" and as much work or exercise as might be possible in others as well as in himself.

Of the "two institutions, the first of their kind in this country," the Adirondack Cottage Sanitarium was, perhaps, the most immediately important to the tuberculous sick. Its growth is, perhaps, hardly to be spoken of much more here as it, and the associated institutes, are still useful and still growing. The point which makes the history of St. John's in the Wilderness so important to us is that *it taught Trudeau to beg*. The wonder-

ful power which he developed in this direction gave him gradually the growth of a sanatorium at Saranac. "The idea of building the Sanitarium originated in my reading, in 1882, in Anstie's *English Practitioners*, which Dr. Walton sent me regularly, an account of a visit to Brehmer's Sanitarium in Silesia." Thus the work of Brehmer and of Dettweiler came to be a guiding principle to Trudeau in the combination of his own experiences with those of others in the same field. "I was also much impressed at that time with the difficulty of obtaining suitable accommodations in the Adirondacks for patients of moderate means." With the help of Mr. D. W. Riddle, who came to the Adirondacks as a very sick man and gradually got quite fit again under Trudeau's guidance and accepted the post of treasurer, in which he was succeeded by Mr. George S. Brewster at his death thirty years later, the affairs of the money side of the project were gradually put in order. "In the summer of 1882," Trudeau says "I again met Dr. Alfred Loomis at Paul Smith's and told him that it seemed to me too bad, owing to high prices and lack of cheap accommodations, that some of the poor sick people in cities could not have the chance of improvement that I had had by coming to the Adirondacks. I then unfolded my plan of building a few cottages at Saranac Lake. . . . He approved and said he would be glad to send me such patients as they applied to him in the city and that he would examine them free of charge."

The first subscription he had came from Mr. Anson Phelps Stokes to whom he had divulged his plan when returning from an expedition in the latter's boat, the *Delos*. This gentleman was much struck with it and said that Trudeau might "call on him for five hundred dollars at any time." From that time Trudeau "got a little book and solicited subscriptions at every opportunity" and the money came in fast, from quite small subscriptions to very large ones like that of a certain Mr. James who "took out his pen and wrote down his name for twenty-five hundred dollars." The money being now available, he determined to choose a site and start the adventure. He finally selected one, "a little level piece of ground, my favourite fox-runway, where I had spent many days while hunting with

Fritz Hallock, which was always perfectly sheltered from both the south and west winds." Is it not touching to find him always taking the hunting qualities of places into account? Nor was he wrong in this practice. "The guides gave me a most pleasant surprise by purchasing sixteen acres of 'Preacher Smith's' pasture (the coveted site) and presenting it to me for the purpose I had in view."

"Late in the fall Dr. Loomis sent up the first two patients, two sisters, both factory girls; one, Alice Hunt, had pulmonary tuberculosis, and the other, Mary Hunt, had Pott's disease and now showed slight evidence of pulmonary tuberculosis as well." These two, poorly clad for the climate and very ill, were taken in as members of the family by the Nortons, people whom Trudeau had gathered to run the beginnings of the sanitarium, and he himself gave them both advice and treatment and encouraged them as far as he was able.

Just think of this arrival of two poor girls as the first flight of what was to prove a great multitude! There is something infinitely touching in their arrival and their treatment free of charge! Looking back in after years, Trudeau could see the sanitarium, as it was then, "The grounds were a rough hillside covered with scant grass, through which everywhere jutted boulders of various sizes, a few rising four or five feet above the ground. Not a sidewalk, not a pathway anywhere! The buildings, a small rough-board and shingle barn, one unpainted wing of the main building without any porch, and one small unpainted cottage!"

And, thirty years after, Dr. Herman Biggs was to say of it: "Doctor I think it is the most beautiful institution of the kind I have ever seen!"

The difference is this; most sanatoria are built to a definite plan and are not occupied until they are finished; whereas Saranac was built as patients and money accumulated, was occupied from the very first, and was everywhere constructed for the momentary advantage and convenience of the patients.

The story of Saranac Sanitarium is the story of Trudeau's own life as it passed in a continual round of work. Most people live a life of work but at the end there is nothing to show for it.

The Saranac Sanitarium stands as the epitome of Trudeau's life and it is curious how one realizes this as one visits it. Here he lived in and for the tuberculous patients; here he lost his darling daughter, Chatte; here his eldest son was bred up, came to maturity, married, and then died suddenly; here Robert Louis Stevenson came for a space and left an abiding memory.

The Saranac Laboratory

In 1882 Robert Koch discovered the tubercle bacillus and how to stain it; and not merely how to stain it but how to grow it in pure culture and infect animals with it in eternal chain. The news of this discovery came only as a gradually appreciated truth to the New World and amongst the first to realize its importance was Edward Trudeau.

Unfortunately, however, he could not read German and only perceived the great discovery through reports in the medical press. "I confided all this to my friend, Mr. Lea, and sorrowfully told him as I could not read German there would be no use in my trying to obtain Koch's paper." What was his surprise and delight when Mr. Lea obtained somebody to transcribe Koch's paper and presented it to him with a thoroughly good rendering of *The Etiology of Tuberculosis*, in English, as a Christmas present. "I read every word of it over and over again . . . It is certainly one of the most, if not the most, important medical papers written . . . every step proved over and over again before the next step was taken, and the ingenuity of the new methods of staining, separating and growing the germs read like a fairy-tale to me." But it was not enough merely to read of these discoveries. It was necessary, to a man of Trudeau's kind, to learn to apply and, perhaps, extend them for the good of his patients at Saranac. "I had my microscope, however, and I decided the next time I went to New York to devote all my efforts to learning to stain and recognize the tubercle bacillus." I consulted all my physician friends as to who could teach me what I wanted to learn but none of them knew or took any interest." Through a Dr. Peabody, however, he heard of the one man who could help him and he ended by calling on a Dr. T. Mitchell Prudden who was teaching pathology at the College of Physicians and Surgeons.

Prudden was then working at, perhaps, the most important section of medicine, under the worst possible conditions of accommodation, as seems to have been the fate of most pathologists at that time.

Trudeau climbed the ladder which led to the sanctum of the Professor and met, for the first time, the man who was to initiate him into the method that he sought. Although apparently rather short in his manner, as a man bothered with the numerous calls on his time naturally occurring to a pathologist, Dr. Prudden had a striking personality and at once appealed to Trudeau. Thus began a friendship which was destined to grow and bind these men together in much future work. "Yes, I could come to the laboratory and he would teach me how to stain the tubercle bacillus . . . he would ask Dr. Hodenpyl to show me how to make the stains. I got a microscope and a place at the long table. I was given a specimen said to contain the tubercle bacillus and Dr. Hodenpyl showed me where the stains were and wrote some simple directions for each step to be taken." So Trudeau was set up with the necessary appliances. But though he worked hard for three days he worked in vain. Then Prudden, struck with his persistence, came and sat down by him, corrected his mistakes, and showed him the right way, so that, at last, he succeeded and there, before his eyes, was the long-sought bacillus. Trudeau stayed very late that night and made perfectly sure that the bacillus should always respond to the method that had been shown to him, and he finally learnt thoroughly "what to avoid doing to insure success." With these principles thoroughly laid to heart he returned to his little laboratory at Saranac, a small wooden shack, next to his house, and there was able to detect those suffering from tuberculosis by the only certain way, the finding of the tubercle bacillus. "When I returned from New York with my newly acquired knowledge . . . I began at once to equip my small office in the Queen Anne cottage—a room twelve by eight feet, having two small closets at one end—with what simple apparatus I could devise and procure." This space, however, was so cramped that he was obliged to build a little addition to his office and this became his laboratory until it was burned down, seven years later, through a

fire originating from Trudeau's thermostat. Here his techniques were developed and even went so far as to include the culture of the tubercle bacillus, the first occasion on which this had been accomplished in America. "I bought" he says "a small sheep for three dollars and a half and from the sacrifice of this animal I procured the required amount of blood which, thanks to the pure air and the snow on the ground, remained tolerably free from contamination and was transferred at once to the ice box to coagulate." With the serum thus obtained he made preparations from the tuberculous gland of a guinea pig and placed the tubes in his home-made thermostat. Most of these were contaminated, but four of them remained sterile until, on the eighteenth day, one of them showed a little growth at the corner. From this he made slides and one can imagine his delight when it turned out to be a pure culture of the tubercle bacillus. This culture infected guinea pigs and rabbits and, in short, left Trudeau in possession of a pure strain of the tubercle bacillus to be used as long as he might think fit.

"In December, 1892, a slender and pale young man rang my door-bell one morning and told me he was a doctor, had contracted tuberculosis, and wanted to go to the Sanitarium. . . . He told me his name was Edward R. Baldwin, that he was from New Haven; and, when I asked him what made him think that he had tuberculosis, he quite floored me by his answer; that he had used his microscope and knew he had it." Thus, out of the blue, came the skilled physician, the wonderful assistant, the dear friend and comrade, that Trudeau wanted. "Through many long years of friendly fellowship, through many long years of work side by side, through many long years of physical suffering, my debt to Dr. Baldwin has steadily grown." So speaks Trudeau and so, indeed, was this "slender and pale young man" to prove himself in Trudeau's service. He has only recently died, in the year 1947, the last great figure of that prolific period to pass away. With his help Trudeau was to accomplish wonders and to make the little laboratory, and the big one of brick and stone that took its place, known to fame throughout the whole world. Trudeau and Baldwin together made a combination which has seldom been equalled. From their joint

work was to go forth the inspiration which still runs on in the person of Opie, the inspiration which kindled in the brain of Allen K. Krause and has flowed so fully to Arnold Rice Rich and others.

But it is necessary to return to a period before Baldwin's arrival to examine a few of the experiments which Trudeau carried out at his little laboratory and which seem to require mention here. First there was an experiment with rabbits which seems to me to have been of great importance. Trudeau took three lots of five rabbits each. To one lot he inoculated pure cultures of his recently isolated tubercle culture and then placed them under "the best surroundings of light, food and air obtainable." These five rabbits, with one exception, did very well and completely recovered. To a second lot of five, he gave a similar dose of tubercle culture and then placed them under the worst conditions possible, confining them in a small box in a damp dark place where the air was bad and the food insufficient. The third lot, uninoculated with culture, was placed in exactly the same conditions as the second. Lot two reacted very badly and four of the five rabbits died with extensive tuberculosis. Lot three, although the rabbits all showed signs of debility, remained free from tuberculosis on post-mortem examination on being killed. Now this experiment, carried out with human tubercle bacilli, from which rabbits, well kept, usually recover, was of great interest as showing that such rabbits, ill kept and under the worst sanitary conditions, tend to do very badly. These unfavourable surroundings, however, failed to bring about death in the uninoculated rabbits though they led to debility. The deaths of four out of five of lot two were due to the lighting up of this disease under unhygienic conditions. This finding, though its full significance was necessarily hidden from the experimenter, shows clearly why the least prosperous classes of human beings get a much more fatal type of tuberculosis than the better endowed. It has also some bearing on the good results of sanatorium treatment as Trudeau thought. It was not until Theobald Smith had differentiated between "human" and "bovine" that the greater significance of this experiment was to be fully appreciated. It

remains to be said that if the rabbits had all been inoculated with "bovine" bacilli they would all have died, whether kept under ideal conditions or exposed to the worst. It was fortunate that Trudeau gave his rabbits "human" bacilli which cause, in that species, the sort of tuberculosis which may be either recovered from, under good conditions, or activated under bad.

It seems, too, that Trudeau had produced tuberculin even before Koch had published his discovery of his "cure" for tuberculosis which, in the event, proved not to be a cure! "I began" he says "to realize about this time that the direct destruction of the germ in the tissues by germicides was a hopeless proposition and, inspired by Pasteur's work on anthrax, chicken cholera and hydrophobia, I sought to produce immunity in my animals by dead germs, *or preventive inoculations of substances derived from the liquid cultures from which the bacilli had been filtered.* I published this work in the *New York Medical Record* as early as November 22, 1890, describing my experiments in detail and giving my conclusions that neither the dead germs nor the soluble poisonous substances derived from liquid cultures of the tubercle bacillus protected rabbits and guinea pigs against subsequent inoculations." Now Koch, although he announced his discovery of a "curative" substance in August, 1890, did not publish his methods of making tuberculin until January, 1891, so that Trudeau had the priority and, also, the discovery which Koch was only to make later if at all, that this substance was of but little value as an immunizing agent. Allen K. Krause has said that this discovery of tuberculin and its relative usefulness in producing immunity by Trudeau was really a greater discovery than that of Robert Koch. This, however, is open to question. Koch, it must be admitted, was the great investigator who discovered, named, described this substance and, although he was wrong as to its curative and even about its immunizing qualities, was the originator of the tuberculin which is used today as the most delicate test for the presence of the bacillus within the human or the animal body. In giving the palm to Koch, however, we admit that Trudeau, at about the same time found a substance which modern investigation declares to be of the

same nature as tuberculin; found it, found that it was of very little value as an immunizing agent and, therefore, did no more about it. This, though a very important bit of research, hardly entitles him to the place in which Koch's intense study and profound thinking placed him with regard to what he called and what we still call "tuberculin."

What, then, shall we say of Trudeau's work? We can and must say that he was a *genius*. Like a genius he appreciated the true significance of the infection of the human body by the tubercle bacillus; its persistence, its setting-up of relative or of absolute disease; its production, in the latter case, of a condition that limits or destroys life, its amenability to rest and its lethal response to over-exertion. Like a genius, too, he gradually marked how the disease could be controlled in his own case and set up an organization which could control it in others. And, surely, too, we may describe his development of the Saranac Laboratory and his reproduction of the findings of Koch as the work of a genius; not, perhaps, the supreme work of a great master but the extraordinary virtue of a disciple in the cause of truth.

Allen K. Krause speaks of the difference which divided the Adirondack Cottage Sanitarium from all others and adds: "It is as true as it can be that what made it different was that its founder was a scientist!"

His name, says Krause, "will be invoked to put before young doctors and old the pattern, the model, of the abounding humanity of Medicine." Let us leave it at that.

Trudeau, then, lived on to a ripe old age at Saranac; lived on to see the place which he had chosen grow into a small town, full of cottages where the tuberculous might seek shelter near one who had come to be regarded as the greatest authority on treatment. And the sanitarium grew also; now a hive of activity in the reception and the management of favourable cases; gaining, under the superintendence of Dr. Lawrenson Brown, ever fresh laurels for the wisdom of the man who had founded and inspired such an institution. "How little I knew" says Trudeau, speaking of the time when he first arrived at Paul Smith's, "that forty-two years later most of them," the guides

and sportsmen who helped him up to his room, "would be dead and that I should still be in the Adirondacks and trying to describe my first arrival."

But everything comes to an end! On the 15th of November, 1915, Trudeau laid down the heavy burden of a long life spent in the service of others and left American physicians the secret of how to manage and how to begin to treat the most formidable of all foes to humanity. They have taken his advice and there is no place where the tuberculous are so well "managed" and treated as in America today.

ROBERT KOCH (1843-1910)

HOW is one to write a Life of Robert Koch without being familiar with his language? How can one whose want of a good knowledge of German cuts him off from all the subtleties of the best "Lives" written by his fellow-countrymen hope to do justice to him? Alas, it is too late for me now to acquire German. All I can claim to the understanding of the works and writings of Koch is a profound admiration, real sympathy and a deep appreciation for all the difficulties, the struggles and the final triumph that were to star its course with glory.

Let us commence with a French *nécrologie* in the *Annales* of the Pasteur Institute (24. 1910). "Les disciples de Pasteur tiennent à saluer ici même dans ces Annales consacrées à la microbiologie la mémoire de ce grand homme, à dire leur admiration pour son œuvre et les regrets que cause sa perte. Koch est un des fondateurs de la bacteriologie; il n'a qu'un predecesseur, Pasteur."

These noble words, especially coming from the Pasteur Institute where he might so well have been regarded as an enemy, constitute a real tribute to the superlative genius of the work of Koch. I place them at the beginning as a demonstration of how the wonderful attainments of Koch were appreciated by a noble

people with whom he had not hesitated to give some cause at least for resentment!

The next authority whose work I shall quote is Dr. Emile Lagrange, a Belgian writer whose excellent life of Robert Koch, "sa vie et son œuvre," (à l'Édition Universelle, S.A., Rue Royale, 53, Bruxelles) is one of the few accounts of him extant in English or in French (1938).

It was at Clausthal, says Lagrange, a little town in the Harz, that was born on the 11th of December, 1843, the future head of German bacteriology, Robert Koch. His father, a man of great intellectual culture, was *Bergrat* and Chief of a Service. His mother, a simple and uncomplaining woman, was an excellent head of a large family of eleven children, most of whom went to America, one became *Bergrat* in his father's place and one, Robert, took up medicine. The latter took his medical courses at the University of Göttingen, a little town with a great and illustrious Medical Faculty. He worked at Göttingen for four years, carried out his medical and scientific studies, and left with the mention, *Cum extrema lauda*.

Koch passed his medical examinations and his doctorate in 1866. He became, for some months, Assistant in Pathological Anatomy, and was evidently an exceptional student. He then went to Berlin where he studied under Virchow for a time and then returned to Clausthal where he tried his hand at private practice. He found this very uninteresting, however, and so accepted an appointment as a doctor at a Hamburg hospital where he married, in 1867, a friend of his childhood, Emily Fraatz, daughter of the Intendant at Clausthal, finally settling down with her at Rakwitz, a little town in the *arrondissement* of Bomst. In 1870, being turned down for the army on account of defective sight, he carried on with his work but, in 1871, during the Franco-German war, he again volunteered and, this time, was accepted and posted to the "second line" in Lorraine and, finally, to a military hospital close to Orléans. Lagrange says of him that he was now in a state of the greatest happiness! "Il est tout à l'enthousiasme de la victoire de son pays. C'est la guerre fraîche et joyeuse." That is how a young man feels about it provided he is in the "second line".



ROBERT KOCH (1843-1910)

and that the more frightful and horrible side of war is denied him! "C'est la guerre fraîche et joyeuse." It was now time for him to settle down into a definite job. After some hesitation he decided to settle in Wollstein, a neighbouring town, with the appointment of *Kreisphysiken* or Sanitary Officer of the Centre, and held the post from 1872 onwards. This appointment he obtained "thanks to the recommendation of the *Landrat*, his high status as an army medical officer and his good reputation as a doctor at Rakwitz."

Henry E. Sigerst, in his book, *Great Doctors* (1937) says of him: "On the German side a young physician, Robert Koch, had taken part in the war as a volunteer. He was twenty years younger than Pasteur and sprang from a miner's family. In 1872, the war over, Koch settled down as District Medical Officer at Bomst in Wollstein, a place of 4,000 inhabitants." Here he seems to have been a thoroughly intelligent type of medical man, to have reported, as in duty bound, on epidemics and epizootics, and to have carried out a great deal of Jennerian vaccination. He made many friends, took a great interest in prehistoric finds, wrote to his old master, Virchow, about them, and so awakened the old man's interest that he appears to have come to Wollstein to visit them with his pupil, who seems, too, to have become a "naturalist," or a collector of creatures for his experiments or, perhaps, as is often the case, "a bit of both." Certain it is that he collected and kept numerous wild mice from about the house, an aviary, places for dogs, cats and monkeys, which latter he used for his experiments on relapsing fever, and installed a regular laboratory where he worked with the microscope which his wife had given him—a Hartnack—on one of his birthdays. Here, then, is the man, equipped for any kind of individual research; and, close to him, at his very elbow, the subject for research! "Le Charbon, que les allemands appellent milzbrand."

It is probable that, in his studies of epizootics, this one came most urgently to his notice. There can be no doubt that he was well aware of all that was to be known of former studies of anthrax; the work of Pollender in 1842, that of Davaine and others in 1863; and he must have found the non-motile rods and

the long threads in the blood of sheep dead of the disease. To such a man as Koch, the mere possession of a microscope makes this as certain as possible.

Let us turn from these considerations to the delightfully irresponsible and yet very nearly true utterances of Paul De Kruif (*Microbe Hunters*, 1926).

Koch is imagined talking to himself as he gazes at the red corpuscles of a sheep dead of the disease and notes the presence of the anthrax bacilli amongst them. "These threads and rods are never found in the blood of healthy animals" he pondered. "That is all very well but it doesn't tell me whether they are bacilli, whether they are alive! . . . It doesn't show me that they grow, breed, multiply!" Koch bought a number of mice! (Lagrange says that, *he had a number of mice* in his study, caught about the house.) Of these, after the manner of De Kruif, he soliloquizes: "I haven't the money to buy sheep and cows for my experiments . . . but maybe I can give anthrax to the mice . . . maybe I can prove that the sticks really grow!"

These thoughts, if they were ever formulated, were prophetic. "He took slivers of wood, cleansed them carefully, heated them in an oven to kill any chance microbes that might be sticking to them. . These slivers he dipped into drops of blood from sheep dead of anthrax; blood filled with the mysterious motionless threads and rods . . . then he made a little cut with a clean knife at the root of the tail of the mouse and into this cut he delicately slid the blood-soaked sliver! . . . and next morning Koch came into his home-made laboratory to find the mouse on its back, stiff!"

Still, though revelations in the history of mice and men went on progressively, there must have been domestic crises! "Worst of all, when he came into the living room from his mess and microscope to tell Frau Koch about the new strange things he had discovered, this good lady wrinkled up her nose and told him: 'But Robert you smell so!'"

This, if, it ever happened, must have been galling, but yet he persisted with his observations. "I will try" says Koch "to make these threads multiply in something that is as near as possible like the stuff an animal body is made of. It must be just

like the living stuff." He put a little of the spleen of a mouse dead of anthrax into the watery fluid from the eye of an ox. "I'll put them in a hanging drop . . . nothing can get into that drop. Only the rods are there . . . Now we'll see if they will grow." Let us recall that the "hanging drop" which we use so freely today was invented by Koch during this very experiment!

"The drifting rods had begun to grow. Here were two where one had been before. There was one stretching itself out into a tangled endless thread."

"Then each day of a nervous test that lasted eight days Koch repeated his experiment of making a million bacilli grow where only a few were before. He planted an even drop of his rod-swarming hanging drop into a fresh, pure drop of the watery fluid of an ox-eye and, in every case, out of these new drops the few rods grew into myriads."

His hanging-drop experiment had been a wonderful success.

He stops at the eighth experiment and, once more, soliloquizes: "I have grown these bacilli for eight generations away from any animal; I have grown them pure apart from any other microbe—no diseased tissue in this hanging drop! Will these bacilli grow in a mouse?" He contaminated a sliver from the hanging drop . . . then deftly slid this splinter under the skin of a healthy animal." The mouse was dead next day. It was dissected and slides made from the enlarged spleen. "I've proved it," he whispers, "here are the threads, the rods."

Nor was this all. By keeping his hanging drops for an appropriate time he made yet another discovery.

"The outlines of these threads had grown dim; and each little thread was speckled through its whole length with little ovals that shone brightly like glass beads." He tried keeping them for varying periods. The result was always the same. "Spores." "That must be the way the anthrax microbe can keep itself alive in the fields for so long . . . the bacilli must turn into spores."

From this he passed naturally to the conclusion: "All animals that die of anthrax must be destroyed at once after they die . . . burned or buried very deep." The idea of the spores being

brought to the surface in the casts of earth worms had not occurred to him. This notion, whether true or not, must be attributed to Pasteur.

All these extracts from the work of Paul De Kruif, very largely we think, the result of the play of his imagination round the solid and painstaking work of Koch—the inspired work, we feel inclined to say—serve wonderfully to bring to the mind of the reader all the thoughts and fancies, all the soliloquies and comprehensions, that mark out the exceptional bacteriologist from the more common man. The result may not be serious science but it serves, at least, to bring into immediate notice the difficulties that undoubtedly beset him. After all, we are not here concerned with more than to introduce Robert Koch as not a common man but an exceptional and self-taught striver after truth, whose wonderful researches into the nature of *tuberculosis* are still to come and, without this preliminary training in bacteriology, would not be conceivable.

For this he must have placed on one side the pleasant companionship of intelligent friends, the thoughts and pursuits of an amiable country medical officer. As Lagrange says: "The studies of palæontology are dead and gone; the parties of *échecs* and the soirées among friends; these, too, disappear. Koch throws himself, with head lowered like a bull, into the study of *charbon*."

The subsequent career of Robert Koch was a wonderful transit from the situation of a young man of comparative obscurity to the chief of all the German scientists in the struggle against disease. In 1876, on April 22nd, still in his larval stage but having grown wings that were to bear him upwards, he applied to Professor Cohn, an old friend and instructor, to join with him in his studies of anthrax. The result was an invitation to Breslau, a session in Cohn's own department to which many prominent doctors were invited, the opportunity for Koch to tell his story and show his specimens to such men as Conheim, Weigert, Traube and others, an immediate success and a ready recognition that here was a striking research, done by Koch alone and without the aid of any scientific body to help him; a problem faced alone, studied with consummate art and solved by processes

specially and originally designed to bring the truth to light from its darkest hiding place.

Conheim himself, the discoverer of an unerring method of demonstrating the infectivity of tuberculosis by the insertion of tuberculous matter into the anterior chamber of a rabbit's eye, rises from his chair in the greatest excitement and rushes to where his students are working: "Leave all your researches, all of you, and go to see Koch's demonstrations! This man has accomplished a great thing which, in its simplicity and precision, merits the highest appreciation from us all." Within a month, actually on May 29th, 1876, Koch had sent to Cohn's paper, *Beiträge zur Biologie der Pflanzen*, a memoir entitled "Die ætiologie der Milzbrand-krankheit begründet auf die Entwicklungsgeschichte der *Bacillus Anthracis*." This was actually the first verified account of a living germ as a cause of a disease and an explanation of its method of spread. As Koch said, in closing the paper "Durch die heurbei gewonnenen Resultate und Unter-Suchungsmethoden müssen wir uns dann den Weg zum Ferneren und uns zugänglicheren zeigen lassen." Well it was for Koch that he had fallen into the generous hands of men like the great Cohn, the still greater Conheim and the fraternity of physicians at Breslau.

He was invited to come to Breslau as "City Physician," a post permitting private practice, and he accepted this opportunity to get to a place where he was already sure of a welcome and of the means of increasing his stock of knowledge. Here, for some years, he continued to work and to gain a familiarity with disease from the bacteriological point of view, putting forth his now well-known "postulates"; the *sine qua non* of the attribution of a germ to a disease:

1. The parasite must be found in every lesion of the disease.
2. It should be cultivated pure, outside the body, for several generations.
3. After pure culture, for sufficient time and for several generations, it should reproduce the original illness in the body of a laboratory animal.

His fame grew; he had many competent or in-competent

followers; his methods were beginning to make their way in the hands of the many. In 1873, a site for a research laboratory had been purchased by the State at Louisestrasse 57, Berlin, for 200,000 marks, and had been re-fitted for a further sum of 110,000 marks, as the Imperial Office of Health. The staff had consisted of a veterinary officer, two hygienists, a chemist, and some details. In 1880, *Geheimrat* Dr. Fenkenbaum retired owing to age, and both Cohn and Koch were considered for the appointment in his place. Finally Robert Koch was appointed; and, shortly after, two colleagues were given him, Loeffler and Gaffky, both military doctors, as well as a large laboratory, while he himself was nominated as Extraordinary Member of the Imperial Office of Health of Berlin. These three started at once the *Mittheilungen aus dem Kaiserlichen Gesundheitsamte* in the first volume of which Koch published his great discovery of the making and keeping of pure cultures on solid media. This was the outcome of what Koch admitted was a pure accident, the growth of a mould on a portion of potato left in his laboratory; an accident which might have been ignored by any but one with the prepared mind! It led Koch to his great discovery, however, for he had, in the highest degree, the mind to notice and to ponder over what lay before him. What was this growth and why was it not contaminated? He took a portion of it; stained it; found that it was quite pure; tried the effects of placing other growths on nutrient media hardened with a small quantity of gelatine placed under sterile glass; and, behold, he had found the way to grow germs pure, away from all other germs!

The Tubercle Bacillus

In 1881, Koch returned from the Congress of London and, at once, determined to hunt for the germ of tuberculosis. He cast about him for the previous work on the subject and came to the following conclusions: "The work of Villemin has left undecided the question of knowing for certain if tuberculosis is an infectious malady," Here Koch must have been under the influence of a racial dislike which led him to ignore what we now recognize as a wonderful and convincing piece of work which had demonstrated for ever what Koch now ignored. "But"

he continues "the inoculations of Conheim and Salomonsen and, later, Baumgarten, have made the thing certain." These researches, depending on the infection of a rabbit's eye by placing tuberculous material in the anterior chamber, were, of course, quite as convincing as Villemin's, the tubercles growing and spreading to all parts of the body, and we fully concur with their mention though they were a little later, in point of time, than Villemin's observation. Here, though it is, perhaps, ungenerous on the verge of describing Koch's great discovery, we feel that we must mention the slightly vulgar and strictly racial attitude of Robert Koch to both Pasteur and Villemin, whose work he ignored or belittled; but it is possible to forget it in the great admiration which we feel for his own wonderful contribution. Koch, like all who had hitherto searched for the tubercle bacillus, was, at first, quite unable to find it! "With regard to tuberculosis" he says "it was to be expected that the discovery of pathological organisms might be attended with unusual difficulty, since many attempts had been made to demonstrate them without producing satisfactory results. I began my investigations with material in which the infective virus might confidently be expected to exist, for example, recently developed grey tubercles from the lungs of animals killed three or four weeks after inoculation. Sections of such organs hardened in alcohol were prepared and examined for bacteria after the most trustworthy methods. Grey tubercles were crushed, spread on cover glasses, dried and then examined for the presence of micro-organisms. Every attempt to discover bacteria or other micro-organisms was unsuccessful." (Translation by Stanley Boyd, F.R.C.S., of the *Mittheilungen aus dem Gesundheitsamte*, by Robert Koch, Vol. II, 1884, in *Micro-organisms in Disease. Selected Essays*. New Sydenham Society. 1886. Selected and edited by W. Watson Cheyne, M.B., F.R.C.S.)

He goes on to the method by which he ultimately stained and demonstrated the bacilli:

"Earlier observations having shown that in certain cases the deepest staining and clearest differentiation of bacteria from surrounding tissues were yielded by the use of stains which

were of alkaline reaction, advantage was taken of this fact. Of the common aniline dyes, methylene blue bears the freest addition of alkalis, therefore this staining material was chosen; and to a watery solution of it, caustic potash was added so long as no precipitate formed and so long as the fluid remained clear. For this purpose 1 c.cm. of a concentrated alcoholic solution of methylene blue was mixed with 200 c.cm. of distilled water and well shaken. To this, 2 c.cm. of a 10 per cent solution of caustic potash were added, the mixture being repeatedly shaken. When the cover glasses were exposed to this staining fluid for 24 hours, very fine rod-like forms became apparent in the tubercular mass for the first time, having, as further observations showed, the power of multiplication and of spore formation and hence belonging to the same group of organisms as the anthrax bacillus. It was incomparably more difficult to recognize these bacilli in sections among the heaped up nuclei and masses of detritus, and an attempt was made to render the tubercle bacilli more evident by contrast-staining according to the method by which Weigert succeeded in colouring splenic fever bacilli with one tint and the surrounding tissues with another. This object was attained by using a concentrated solution of vesuvin with which the blue-stained cover glass preparations and sections were treated until they appeared perfectly brown to the naked eye. Microscopic examination then showed that only the previously blue-stained cell-nuclei and detritus became brown while the tubercle bacilli remained a beautiful blue."

They were easily recognized, even among masses of nuclei, and the experiment was, in fact, a complete success. "In the above process, methylene blue does not colour the bacilli very deeply and some practice is needed to demonstrate them successfully in all the tubercular organs" adds Koch; but we have tried this procedure and found them very well stained; almost as well as with the Ziehl-Neelson technique.

"Recourse must be had to the best optical aids," says Koch, "oil-immersion lenses and an Abbé's condenser. As to the magnifying power necessary in examining for tubercle bacilli I may

say that 500 or 700 diameters is the most useful and can be obtained by the use of $\frac{1}{12}$ -inch oil-immersion objectives and the corresponding eye-pieces." It will be seen, that he had arrived already at the type of magnification which we now employ. He discovered, too, that the leprosy bacillus was equally stainable and by the same processes. "The instance of the bacillus of leprosy shows that the tubercle bacillus holds no strictly exceptional position in its behaviour towards colouring materials, and it is, therefore, not improbable that in time other bacteria may be discovered which have the same staining properties as the tubercle bacillus."

Koch was, then, prepared for discoveries in the acid-fast group and he has proved to be right; but it is still very exceptional to find any other acid-fast in the human sputum than the bacillus of Koch, though others may occasionally occur. I have myself seen a case where a strange and rare acid-fast bacillus other than the tubercle bacillus led a very distinguished consultant to give an unfavourable prognosis which was only abandoned when laboratory tests showed the true nature of the germ; the furnisher of which is still alive and well after over ten years.

Koch's illustrations of the tubercle bacillus, the giant cell, the culture, are all now rather the commonplaces of medicine than the teeming riches of a wonderful discoverer, but they came to a world which had not yet seen anything like them and caused the most wide-spread discussion and the most intense astonishment—for many good bacteriologists were able to verify the findings of Koch as soon as they had read or heard his descriptions of his technique and seen what he had to show them.

Koch turned to the finding of the bacilli in every kind of tuberculous case; they were invariably present when skillfully sought for and vigorously pursued through numerous preparations but, as a matter of fact, they were readily found in most instances and afforded a positive reaction in all ordinary cases without much trouble. It is only with the discovery that the *earliest* cases may slip through without anything but a successful culture or the inoculation of one or more guinea pigs that the finding of tubercle bacilli becomes such an intensely difficult

and such an important matter. But here, too, we are indebted to Robert Koch.

Koch examined many cases of tuberculosis and found the germ in them all except two cases, one a case of renal abscess and the other an abscess due to spinal caries. These he thought to be instances in which the bacilli, present at first, had, by the time when the specimens were examined, disappeared; as indeed they probably were. He concludes: "The number of individual cases which were examined, considered collectively and with special reference to the separate forms of tuberculosis, was large enough to justify the conclusion that we were dealing with a constant, not a chance phenomenon, and that the tubercle bacillus, therefore, is a typical element in tubercle and its products." These conclusions were, of course, eminently justified. He also carried out many examinations of animals with the same results as in man. His next great experiment was in the cultivation of the bacillus which he had found to be present in practically every case of tuberculosis and in nothing else. "From the beginning the method of cultivation [of tubercle bacilli] upon a solid transparent soil was employed. . . . First of all an endeavour was made to cultivate the bacilli in a crushed tubercle from the lung upon a nutrient jelly [meat infusion with peptone, rendered solid by gelatine] but without success. These attempts were made at the ordinary temperature of the room as the gelatine liquifies at a higher temperature and thus the advantages of a solid material are lost." These cultures, however, never came to success. "The desired thing appeared to offer itself in solidified blood serum. . . . I had found . . . that when serum is kept for some time at about 65° C., it becomes solid but remains transparent. . . . Substances containing bacilli were now spread out and kept in the incubator at 37° C.; an examination of the preparations under a low power was frequently made and revealed, after a few days, the presence of peculiarly formed colonies which, under higher powers and by the employment of staining processes, were found to consist entirely of tubercle bacilli. . . . Pure cultivations are obtained with the greatest certainty by using as seed a tubercle rich in bacilli or material of the same nature from the interior of lym-

phatic glands, not far advanced in caseation, taken from a recently killed guinea pig. . . . The process must always be carried out with heated instruments which must be changed every time a fresh stratum is laid bare. . . . After the inoculation of the solidified serum with material containing tubercle bacilli has been accomplished, the vessels are placed in the incubator and kept constantly at a temperature of about 37° C. . . . In fluids other than blood serum I could not at first succeed in obtaining a growth of the tubercle bacilli. . . . It was not until I had broken up bits from a culture of bacilli into fine fragments by titration, added them to meat infusion, and distributed them thoroughly by shaking, that an unmistakable development took place. . . . For the success of this experiment it appears to be of some importance to put the cultures into flasks with a flat wide bottom, the so-called Erlenmeyer flasks, and to pour into the flask only enough to cover the bottom to the depth of one half or at most one centimetre."

It would, of course, be ridiculous to attempt to describe the perfected methods which have now succeeded to the earlier technique of Koch, but it is well, I think, to describe his original methods as above because, as will be apparent on reading them, the *essentials* are preserved in every device used today for the staining and culture of the tubercle bacillus.

Under "Infection Experiments with Pure Cultures of Tubercle-bacilli," Koch goes on to prove the validity of his conclusions "by inoculating with bacilli in a pure state isolated from all elements of the body." One experiment, from very many, will be quoted here, merely to show his technique.

"First Experiment. Pure culture from miliary tubercles in the human lung carried through five generations in fifty-four days; subcutaneous inoculation of four guinea pigs; two animals in the same cage not inoculated. In the inoculated animals swelling of the inguinal glands took place after fourteen days, ulceration occurred at the seat of inoculation and the animals began to lose flesh. One of them died at the end of thirty-two days, the others were killed on the thirty-fifth day. The inoculated guinea pigs, the one that died as well as the three killed,

showed extensive tuberculosis of the spleen, liver and lungs; the inguinal glands were much swollen and caseous, to a markedly greater degree on the inoculated side; bronchial glands slightly enlarged. Neither of the animals that had not been inoculated showed any trace of tuberculosis."

This kind of experiment was repeated many times with positive results, the number of generations of the tubercle bacilli outside the body being invariably given to make sure that Koch was "inoculating with bacilli in a pure state isolated from all elements of the body."

Koch took up a sound position with regard to the spread and the localization of bacilli. Here is his interpretation, for instance, of the effects of lymphatic disease on the spread of the infection:

"The almost invariable presence of caseous or calcified bronchial glands in the diseased states just mentioned suggests rather, that the lymphatic glands are not always an insurmountable barrier to the further progress of the bacilli and that, just as single bacilli are carried into the lymph glands by means of wandering cells and the lymph stream, so, in the same way, by the help of the wandering cells, they may again leave the lymphatic glands and, taking a centripetal direction, reach the blood by way of the lymph stream."

It is the case that, as we read the works of Koch, we meet with the most *modern* conceptions, stowed away where the most modern writers, perhaps, have never found them.

Let us turn to one of the questions on which Koch was, in the beginning of his work, uncertain, but on which he came to the truth—and more than the truth—in his later researches. He says himself that he was uncertain about bovine and human tuberculosis, as well he might be, at the start. It is generally held that he considered the two to be identical but, in his own words, he "expressed himself regarding the identity of human tuberculosis and bovine tuberculosis with reserve." (Hillier, A., *The Prevention of Consumption*. London, Longmans, Green & Co., 1903.) Later, however, he came to another opinion, as the

result of further work. "A number of young cattle" he says, "which had stood the tuberculin test . . . were infected in various ways with pure cultures of tubercle bacilli from cases of human tuberculosis; some of them got the tubercular sputum direct. . . . None of these cattle (there were sixteen of them) showed any symptoms of disease and they gained considerably in weight. From six to eight months after the beginning of the experiment they were killed. In their internal organs not a trace of tuberculosis was found. . . . The result was utterly different however, when the same experiment was made on cattle free from tuberculosis, with tubercle bacilli that came from the lungs of an animal suffering from bovine tuberculosis. After about a week the severest tubercular disorders of the internal organs broke out in all the infected animals. . . . High fever set in and the animals became weak and lean; some of them died after a month and a half or two months, others were killed in a miserably sick condition after three months. . . . In short, the cattle proved just as susceptible to infection by the bacillus of bovine tuberculosis as they had proved insusceptible to infection by the bacillus of human tuberculosis. . . . Considering all these facts, I feel justified in maintaining that human tuberculosis differs from bovine and cannot be transmitted to cattle."

In these experiments Koch was right. He drew from them not the definite conclusion but the strong assumption that bovine tubercle bacilli were much less infectious to man than has since proved to be the case. At the British Congress on Tuberculosis in 1901, he spoke up for this opinion as follows: "But now how is it with the susceptibility of man to bovine tuberculosis? This question is far more important to us than that of the susceptibility of cattle to human tuberculosis, highly important as that is too. It is impossible to give this question a direct answer because, of course, the experimental investigation of it with human beings is out of the question. Indirectly, however, we can try to approach it. It is well known that the milk and butter consumed in large cities very often contain large quantities of the bacilli of bovine tuberculosis in a living condition. . . . If the bacilli of bovine tuberculosis were able to infect human beings, many cases of tuberculosis caused by the consumption

of alimenta containing tubercle bacilli could not but occur among the inhabitants of great cities, especially children. . . . In reality, however, this is not the case. That a case of tuberculosis has been caused by alimenta can be assumed with certainty only when the intestine suffers first—i.e. when a so-called primary tuberculosis of the intestine is found. But such cases are extremely rare." Koch, then, while demanding further researches, definitely expressed the view that the danger of bovine bacilli to man would prove very slight. A British Royal Commission was appointed and ultimately proved that this danger was and is very considerable.

This was one of the instances where Koch, absolutely right in the experiments which he carried out and the inferences which he drew from them, was inclined to draw a very serious and erroneous supposition in advance of further observations which, however, he still advocated as necessary before his supposition should be taken as proved. A public utterance, indeed, was not the ideal way in which to air what were, admittedly, only views, and the great position of the speaker in connection with tuberculosis made every shade of opinion expressed by him to be taken as gospel by lesser men.

Koch was widely quoted as having said that bovine tuberculosis was harmless to man whereas he had said no such thing! What he had said was that bovine tuberculosis would probably prove of little harm to man; but he had advocated further research on what he considered to be a subject of the greatest importance. The Royal Commission on Tuberculosis, as well as other bodies appointed to examine the question, brought what Koch had wanted, definite *proof* of the susceptibility of man to bovine tuberculosis; but, at the same time, it proved that Koch's *supposition* had been wrong. The supposition of a lesser man might have been upset far more severely and yet forgotten, but Koch's error of judgement, given forth as it was with such extremely correct experimental researches, was widely taken to be his definite *opinion* and its reversal was greeted with howls of delight by many who could not have conceived or executed the experimental work on which it was based. Even today Koch is said to have been entirely wrong about the relation of bovine bacilli

to human material, whereas he merely demanded further experiment and gave his views as to what the results would probably be—quite wrongly, it is true, but with the clear expression of his real wish that this question should be very seriously examined as one of the most important outstanding problems of tuberculosis. *Koch never asserted that bovine tuberculosis was harmless to man.*

Tuberculin

We come, however, to another question on which Koch did, indeed, conclude that he had found a cure for a disease which is still, in many cases, incurable. "In einem Vortrage, welchen ich vor einigen Monaten auf dem Internationalen Medizinischen Kongresse hielt, habe ich ein Mittel erwähnt, welches imstande ist Versuchstiere unempfindlich gegen Impfung mit Tuberkelbacillen zu machen und bei schon erkrankten Tieren den tuberkulösen Krankheitsprozess zum Stillstand zu bringen." ("In a communication which I made a few months ago to the International Medical Congress, I described a substance of which the result is to make laboratory animals insensitive to inoculations of tubercle bacilli and, in the case of already infected animals, to bring the tuberculous process to a halt.")

Such was his conclusion about his new substance, tuberculin, in laboratory animals. (*Gesammelte Werke von Robert Koch*, Vol. I, p. 661. G. Gafky, E. Pfuhl and J. Schwalbe. Leipzig, 1912.) It is true that he was attempting to prove, in the same chapter that the same desensitization and the same cure might happen in man and, while very hopeful, was not yet absolutely convinced; but there is no doubt that he regarded tuberculin as a desensitizing and curative substance in laboratory animals.

Later, he came to the conclusion that this was actually the case in man and that he had found, in tuberculin, a means of cure of at least the earlier tuberculous lesions in human beings. He was sufficiently sure of this to announce the findings of a remedy to the world and, coming from Koch, this led to a regular rush of all those interested to Berlin to see the results and to take back to their own countries what they could acquire of this new treatment.

Koch was able to prove quite easily that the tuberculin reaction was *specific*. The healthy man was found to give either no reaction at all or only a very slight reaction to 0.01 c.cm. while the tuberculous patient reacted very severely to this amount; the tuberculin being given *subcutaneously* as the substance had proved to be useless when given by the mouth. The reaction, in a tuberculous person, was thus described: "The general reaction consists of a feverish attack which often begins with shivering; the body temperature rises to 39, 40 or even 41° C.; there is pain in the joints, cough, marked languor, nausea and vomiting. Sometimes there might be a slight jaundiced colour and in certain cases there is a measles-like rash on breast and throat. The attack begins, usually, four or five hours after the injection and lasts about twelve to fifteen hours. It may recur and be characterized by a slighter but still definite symptomatology." We see, here, the usual result when too large a dose is given; 0.01 c.cm. in this case; but, even with those who still believe in the therapeutic value of tuberculin, far smaller doses than this are now given with less marked results. Koch tried it in a slightly higher dose on himself. "Die Symptome welche nach der Injection von 0.25 c.cm. beim Menschen entstehen habe ich an mir selbst nach einer am Oberarm gemacht Injection erfahren." Briefly he had a very severe reaction indistinguishable from that given in patients on the injection of a smaller dose. And he quotes the results in four or five other healthy men with doses of 3 or 4 c.cm. which are exactly to type. In other words, he had discovered "allergy," which still remains a well recognized sequel to tuberculin, although he did not use the word.

The same phenomenon was soon described by Koch in guinea pigs; here it was known as the "Koch Phenomenon." If two guinea pigs are taken, the one uninoculated and in good health the other an inoculated animal of some months, and into each of them is inoculated a dose of tubercle bacilli, the hitherto healthy animal at first shows nothing but, in the course of ten days or a fortnight, begins to have large inguinal glands and, gradually, to pass on into the tuberculous process. In the already injected guinea pig, however, there is an almost immediate response to the second dose. There is marked swelling around

the site within twelve to twenty-four hours and this passes on to an open ulcer in many cases. In the meantime the general health of the animal often undergoes a temporary improvement; a change which is far from occurring in the previously uninoculated guinea pig. We see the results of allergy in the previously inoculated pig and the absence of it in the other.

These findings were of great encouragement to Koch who saw in them what he thought to be movements towards cure in the animals which we now recognize as allergic. So, too, were they to the many who had come to Berlin to pick up what they could of the new method of treating tuberculosis.

For Koch was now a very distinguished man; the discoverer of the "comma" bacillus of cholera, the discoverer of the anthrax bacillus and its mode of action, and, last but not least, the finder of the tubercle bacillus and its complicated and mysterious attributes. Any hint of a cure by such a man was sure to be worth following up!

At the same time Koch himself was beginning to withdraw a little from his first triumph and to be rather more doubtful than at first of the invariable success of his own remedy. For one thing, tuberculin could kill a tuberculous guinea pig if given in too large a dose; and he was beginning to see that something of the same kind might follow too large a dose in a tuberculous human being! Then, too, many cases that had at first been apparently on the way to cure after the early doses were still in an unsatisfactory state and resistant to the larger doses that Koch would have liked to advise.

Still there was no doubt of the *diagnostic* value of tuberculin. That was becoming more and more apparent the more often the findings in healthy men and tuberculous patients were compared as to their toleration of a given dose. And it was becoming a more and more valuable *test for infection* in cattle. The end was, for all interested observers, becoming clear. The new substance was becoming more valuable for the *detection of infection*, whether large or small in amount, and its worth for cure becoming less, at least in the hands of the ordinary unskilled physicians. The disciples scattered from their visits to Berlin with hope in their hearts yet with the determination to use nothing but very

small doses in the treatment of cases; and many even began to doubt whether other methods, rest, graded exercise, abundant nourishment, etc, might not be even a more speedy means of dealing with the disease.

Various techniques came and went; the large doses of tuberculin; the small doses of tuberculin; the still smaller doses! At last even the very smallest doses came to be laughed at and treated as a joke. "You take one drop of tuberculin and introduce it into a great butt of water and there you are!" Still, although we have reached a large measure of success with other methods and although tuberculin is generally regarded as useless in the treatment of tuberculosis, there are still competent men who try it even in the moderately chronic forms of pulmonary tuberculosis and there are a great many surgeons who employ it in the treatment of certain cases of genito-urinary disease and of ophthalmologists who believe in it for eye conditions. The numerous forms of modified tuberculin have disappeared and it is only the "old" tuberculin that survives, of the original Koch preparations, the result, on the one hand, of the abandonment or the greatly restricted demand for *treatment preparations* and the very greatly increased requirements of "old" tuberculin for the testing of children, nurses and others exposed for the first time to considerable degrees of infection. And there is the "P.P.D." of Long and Siebert so commonly used in America; a preparation thought to give a more constant and a less complicated variety for such tests. It would be useless, today, to attempt to describe the preparation of tuberculin or the various methods of use; the Calmette method, the Von Pirquet method, the latest and most effectual technique, that of Mantoux, or the more modern results of the tests, since these are the subjects of numerous communications at the present time; but it may be stated without doubt that the use of tuberculin in the detection of tuberculosis in groups such as African and other tribes, the value of it in "surveys," the finding of the first moment of reaction or the discovery of the absence of reaction in nurses and others exposed, in the course of their profession, to considerable risks of infection, has raised a far greater demand for tuberculin now than ever before. It is abundantly clear, in fact,

that the discovery of tuberculin was one of the great "finds" of Koch's life and that, whether a method of treatment of a minor kind or not, it is one of the superlative techniques for the investigation of the disease.

Immunity

There is no guiding principle in the confronting of tuberculosis with facts and ideas more important than the great one of immunity. It enters deeply into all the outstanding differences, otherwise so hard to understand, between the tuberculosis of different races, between the disease in infancy, adolescence and adult life, and between the tuberculosis of males and females. Many doctors, blind to the finer shades of investigation, hold that tuberculosis is a disease into which the subject of immunity does not enter; fortunately Koch was not one of these. In one of his most important papers, "Über die Immunisierung von Rindern gegen Tuberkulose," he speaks as follows: "In folgenden soll daher eine grössere Versuchreihe mitgeteilt werden, bei der es gelungen ist, durch eine bestimmte Art der Vorbehandlung einen hohen Grad von Immunität bei Rindern herzustellen." The paper quoted is in the *Zeitschrift für Hygiene und Infektionskrankheiten*, 1905, Bd. 51, and is from the pen of Robert Koch himself, aided by W. Schutz, F. Neufeld and H. Meissner. "Von den verschiedenen Wegen auf denen man die Immunisierung gegen Perlsucht versucht hat, sind bisher ohne Zweifel die besten Resultate durch die intravenöse Injektion lebender menschlicher Tuberkelbazillen erzielt werden!" Koch set out, then, to establish a great principle; that immunity to tuberculosis really exists and can be exploited; and that far the best way to demonstrate it is to be found in the production of immunity in calves against a fatal dose of bovine tubercle bacilli by the intravenous injection of *human* bacilli into these animals. No doubt he hoped to bring about the saving of many cattle from the disease by immunizing them as calves and was only guided towards the establishment of a principle by the idea that he might be attaining an end valuable in itself in the immunization of cattle. Since that time, however, the use of cows for milking after they have undergone a course of immunization by the injection of

living human tubercle bacilli has been held to be unwise since healthy cattle may pass in their milk the seeds of the bacilli injected to protect them. We shall treat these experiments, then, as establishing a principle rather than as constituting a contribution towards a practical method of immunity in the fight against tuberculosis, although they might be held to possess this quality also. Twenty-six calves were used in the experiment, all except two being immunized with human tubercle bacilli. Of the twenty-four remaining, one died, due, no doubt, to some infection of a vessel in the final virulent injection, as the death was within thirty days of the latter and was due to miliary disease.

On the other hand, eight calves, given an inoculation of from 0.05 to 5 cg. of a virulent bovine strain as a control, all died within about thirty days!

It is not necessary to mention any of the immunized calves here except a group of seven which constitute an excellent demonstration of the induction of immunity. The others, though showing the same protection, are less striking because some of them were killed for autopsy at an interval after their test dose of insufficient time to allow the complete healing of their tuberculous lesions.

Of the seven, all, immunized with 1 or 2 cg. of human bacilli proved to be highly virulent by inoculation of guinea pigs, then, after an interval of about thirty to sixty days with a further dose of 5 cg. of the same, and finally with a *test dose* of 2 cg. of a highly virulent bovine strain at an interval of about ninety days after the second "human" or immunizing injection, remained, after a period of about 280 days, quite well and free from tuberculosis, except that one beast had a hazel-nut-sized consolidation in one lung.

In other words, Koch had conferred upon these animals a complete immunity against a virulent bovine strain which had killed eight calves within thirty days.

The *principle* of immunization had, in fact, been completely established.

We say only that the principle was established because one cannot, obviously, give men two large doses of "bovine" and then a virulent "human" in the hope of providing an immunity

against the latter! For one thing, the "bovine" would not immunize but rather prove fatal in itself, man being just as susceptible to this as to the human variety.

But though we cannot quote experiments in the immunization of man in exact line with these experiments on calves, the principle still applies and is giving good, though restricted, results. The B.C.G. strain, the *Bacillus Calmette Guerin*, a markedly attenuated bovine strain, used for millions of immunizations in France without perhaps much effect but, later, taken up by Norway, Denmark and Sweden with either Weil-Halle's intradermal method of inoculation or with the use of a multi-puncture method of skin infection, has now given proof of its value in reducing if not altogether eliminating the danger of subsequent human contact. It has been employed both in Canada and in the United States with conspicuous success in the case of nurses, students and others and in the immunization of young Indians about to enter upon a life gravely compromised by the risk of tuberculosis. This is not to say that the "immunized" are by any means as completely protected as the calves dealt with by Robert Koch; the very means of immunizing them is on a much lower level. But we are not proceeding too fast and the intravenous method is still regarded as out of reach for man. Perhaps a time is approaching when some variant of the tubercle bacillus, the "vole" bacillus or some other germ to which man proves relatively resistant, will be found by intra-venous inoculation to confer a strong immunity on man just as the human bacillus rendered immune the calves of Koch.

It is strange that in England the B.C.G. vaccine is not permitted owing to its not being possible to guarantee it as conforming to certain rules for standardization; it is a living vaccine and thus not permanently "countable" like the dead vaccines which we permit.

There appears to be, however, a great future for it, as for other living vaccines and, no doubt, England will in the end find some way of approving of such vaccines in the interests of her people.

In the meantime it is becoming more widely recognized that an accidental immunity conferred on all people as they grow up

in the English type of civilization is the main safeguard against tuberculosis, which is rapidly diminishing as we watch its progress. The taking in of those bacilli which are in the air we breath is, in itself, a form of immunization, though liable, because unstandardized, sometimes to give the disease instead of protection against it!

It is the form of immunization which is at present preferred to all others in this country, and it is standing us in good stead to judge by the death returns.

What, then, can we say further of Robert Koch, the founder of the science which we all endeavour to practise? We might follow him in his wonderful investigations of wounds, his far-flung travels in the tracking down of germs, his conquest of cholera, his researches on plague, sleeping sickness, cattle-disease, malaria—nothing seems to have been out of his reach or beyond his scope. We might fill a large volume with these researches and we might, of course, if we felt drawn towards such subjects, tell of his second marriage and his strictures on Pasteur and others.

But we prefer to leave out the manifestations of a merely human personality and we feel obliged, too, to leave untouched the bright *immortality* of his other works in the conquest of disease.

For us, his conquest of tuberculosis is enough! We stand for ever his debtors for this wonderful clearing of the way which has brought the light of truth to bear on the dark and devious journey on which we are bent.

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